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HERMANN VON HELMHOLTZ AND THE NEW PSYCHOLOGY.

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Since the death of Darwin, the loss of no one in the scientific world has made such a deep impression as that of Helmholtz. And this is in keeping with that esteem and admiration which in an ever-increasing measure is accorded to his name in the Old and the New World as well, throughout all scientific circles, and in that more practical sphere of life which in the last decade especially has become so largely dependent upon the services of science. From the early beginning of his career, from the time of the anatomical and chemical studies of his youth, all his researches were directed towards high ends, and were crowned with great success. Whenever he smote the rock of nature, there gushed forth the living waters of knowledge. There have always been and are well-rounded, disciplined minds, authors, philosophers, and others, who are able to speak with easy assurance concerning everything in this world, and of much else besides. But they are not for the most part minds which are themselves productive; they are, on the contrary, mere imitators and makers of books. There have always been, and are, moreover, also men of marked scientific note who have proved productive in various widely divergent fields of knowledge; as, for instance, Thomas Young, who in Optics, no less than in the science of Hieroglyphics, rendered considerable service to his time; or, in a similar way, H. Grassmann. But a truly scientific spirit of extraordinary versa-

tility, and yet at the same time evincing complete unity of organization, with all its ideas harmoniously connected one with another, such the world has indeed produced, but only once in the century.

It would hardly be possible for any one man to-day to succeed in understanding all of Helmholtz' investigations equally well. But each will find a peculiar satisfaction in recalling the special impulse which he has experienced in his own department through the inspiration of Helmholtz. And the representatives of Physiological Psychology should feel themselves impelled to such a retrospect, since it is to that science that Helmholtz dedicated himself at the zenith of his power. The two works which have largely rendered his name illustrious, and from which there has proceeded an incalculable stimulus to other men, belong to our especial province. Without in the least depreciating the thoroughly original and fundamental contributions of E. H. Weber, Fechner, and Lotze, nevertheless it must be acknowledged that these two works, both on account of the scientific consequences which have followed them, and of the general and wide-spread knowledge of their contents throughout the scientific world, have more than all others served to bridge the gulf between Physiology and Psychology — a bridge across which thousands of other men now constantly come and go.

Like Dubois-Reymond, Brücke, Ludwig, Henle, Virchow, Helmholtz also came from the school of Johannes Müller. The latter who, as early as 1822, when still a youth of twenty-one, had defended the thesis 'Nemo psychologus nisi physiologus,' had early broken loose from the chains of the Nature-philosophy of the school of Schelling, although without repudiating altogether the spirit of philosophy. He allied himself with Kant, Spinoza, Herbart. A certain taste for philosophy, at least a friendly disposition towards philosophy, was transmitted to his pupils. But in addition to such an affinity we must also note the mode of exact observation of physical phenomena and the experimental method, which were likewise introduced through Müller into German physiology. As a consequence of this method of re-

search, the scholars rejected the theory of 'Vital Force,' to which the master still adhered; and in this highly important reform of the fundamental conceptions of organic life, fraught with such weighty consequences, lay the principal difference between the new and the old epochs. Helmholtz' first work of the pioneer order, upon the 'Conservation of Energy,' represents this new spirit of inquiry. In the incomparably interesting and stimulating discourse delivered at the celebration of his seventieth birthday, he himself drew attention to the fact that his sole object in this early work had been the critical examination and classification of phenomena in the interests of physiology. On the other hand, the pupils of Müller maintained what was to them the fundamental law in the theory of sense-perception,—the doctrine of the specific energies of the nerves,—although their formulation of it did not include the Kantian elements which were attached to the law as expounded by J. Müller. Helmholtz not only accepted the doctrine in general, but, as is well known, applied it in detail in the spheres of Optics and Acoustics, and maintained the diversity of the specific energies corresponding to differences of quality within one and the same sense. A third generation is now empanelled upon this very hook. Whatever we may wish to substitute for the doctrine, we still find ourselves, in my opinion, involved in great uncertainties and obscurities. But that is beside the point here.

Intimately associated with the above-mentioned theory, as far as the facts go, although not necessarily connected with it, there arose with Müller, and also in his school, the clear and emphatic consciousness of the incompatibility of perception, and of psychological events in general, with the processes of the outer world. Müller had expressed himself, it is true, only in a guarded manner concerning the nature of the soul and its relation to the body; moreover, his ideas upon this subject can be made to agree with those of his followers as little as their's in turn with each other. But in two respects at least they were wholly at one: that psychical activities occur only in strict correlation with the physical, and yet that they are throughout

peculiar both in their nature, and in the minor laws of connection to which they are subject. Such formulas as the following were wholly foreign to their thinking: that consciousness is mere appearance, or really nothing (as the modern followers of so-called 'Parallelism' often express themselves in curious inconsistency. Indeed, the doctrine of the complete reality and unique peculiarity of the psychical phenomena may be regarded as a characteristic feature of that epoch.

While J. Müller, after the appearance of his *Handbuch der Physiologie*, turned his attention more to the development of Comparative Anatomy, Helmholtz' investigations, in accordance with his natural gifts, took quite another direction. He was naturally qualified to be a mathematical physicist of the first rank. While his class was reading Cicero in the Gymnasium, he had been computing the path of light beams through the telescope, and establishing certain propositions which were later used by him in his invention of the Ophthalmoscope. He then became a physician for wholly practical reasons, although by no means contrary to his own tastes; yet after the completion of his great works on psychophysics he again turned his attention to mathematical physics. His most essential reforms in the theories of hearing and sight are due to his command of mathematical principles, in connection, to be sure, with an unusual inclination and aptitude for psychological analysis and with an extraordinary inventive talent for the construction of apparatus. In the latter respect, he himself makes the interesting observation, that his youthful predisposition for geometrical methods of treatment was developed in the course of his many experiments into a kind of mechanical intuition; he could feel, as it were, how the angles and lines distribute themselves in a mechanical contrivance, a peculiarity which is also often found in experienced mechanics and machinists.

The '*Physiological Optics*' appeared in parts during the decade, 1856–66. The studies for this undertaking naturally extended to a much earlier date. The invention of the Ophthalmoscope in 1851 was already an incidental fruit of his exhaustive studies in dioptrics. Then followed the sev-

eral publications, upon the Theory of Color, upon Accommodation, upon the Telestereoscope. The great activity in optical investigations, especially in the fifties and sixties (consider, for instance, such names at Brücke, Dove, Listing, Volkmann, Chevreul, Plateau, Fechner, Brewster, Wheatstone, Maxwell), makes the high standard which Helmholtz' work attained more easily comprehended, and increases our astonishment at the intellectual force which was able to gather under new and quickening points of view this wealth of manifoldly divergent results, both native and foreign, and work them into a consistent whole. We must not overlook the fact also that Helmholtz took scarcely any results from others without independent verification. Then the subject of Dioptrics, which had been to a certain extent already written out, received thorough revision, both theoretical and experimental, at his hands. The methods were perfected in both their applications (Opthalmometer, Microoptometer), all the constants in the investigation of the eye more accurately determined, and the mechanism of accommodation cleared up in its essential features.

In the subsequent portions of the work in which Helmholtz establishes the peculiar distinction between sensation (as of color) and perception (as of space), the following form the principal features: the revival and detailed elaboration of Young's Theory of Color—the first comprehensive discussion of the complex relations of color sensations—and the development of the empirical theory of space. The two theories have passed into nearly all the text-books of Physiology, and have had numberless popularizations; the very best, indeed, of these being by Helmholtz himself, whose *Populär-wissenschaftliche Vorträge* call out our ever-increasing admiration. Throughout the extensive range of the popular science literature of Germany, there are very few counterparts to these lectures. They are unpretentious and yet, while in good taste, sufficiently ornate; they preserve as admirably the golden mean between excessive diffuseness, on the one hand, and too rigid concentration on the other; they are equally free from anecdotes and rhetorical extravagances, and especially free from the common method of first

caricaturing old or opposed views in order to set them aside with superior wisdom as childish and absurd.

Since then, as is well known, the theory of color has been zealously attacked from many quarters, and other theories, especially that of Hering, have been opposed to it. The decision in this controversy is still in abeyance. It would not be becoming for me to enter the lists for either side in this connection. To psychologists, however, the question should present itself more strongly than heretofore, whether the so-called composite colors, in distinction from the primary colors, really result from a number of simultaneous sensations, after the analogy of a chord of tones, or whether, on the other hand, these combined color appearances which originate in one and the same place on the retina, constitute an absolutely simple sensation. If I understand them aright, both Helmholtz and Hering uphold the former view, while the majority of psychologists support the latter, and there has been no adequate discussion of the matter. For each investigator seems to consider his own view self-evident.

An especially attractive part of Helmholtz' theory of color, from a psychological point of view, is his explanation of simultaneous contrast. He here introduces a principle which he also turned to good account on other occasions: that, by virtue of our past experiences, we often come to judge and designate objects of sense as quite different from what they really appear to us at the moment to be.

Notwithstanding the masterly elucidation of this principle, the conviction has now become quite general, principally through the exhaustive researches of Hering, that Helmholtz, through a combination of circumstances, was led to resort to an artificial explanation instead of postulating a simple reciprocal action of contiguous nerve-elements upon one another. This leads us to see that it is the sensation itself which is altered, not the judgment alone. Nevertheless, this principle of explanation is so far forth of value, and it remains an important fact that deceptions respecting the nature of sensation which can be very well distinguished from real illusions, are often brought about through experience.

It was also in this book that Helmholtz first made use of 'unconscious inference' in his explanations, although the idea and the theory itself were first definitely stated in connection with his theory of space. The process through which such false judgments, as that of color-contrast, are produced, seemed to him to be analogous to the process of true inference. The misuse which many naturalists and philosophers, who had felt the influence of Schopenhauer, made of this theory, to bolster up various shallow views, gave Helmholtz an occasion later to revise his view and to substitute for unconscious inference a process of association, which certainly is truer to the psychological facts. Moreover, we must take into consideration the fact that Helmholtz, in the *Physiologische Optik*, expressed himself guardedly and with some reservation concerning unconscious inference. He says: "Although, indeed, the similarity of these psychical phenomena with those of conscious inference has been disputed, and perhaps will continue to be disputed, still the similarity of the results is not at all in doubt."

In the theory of space perception Helmholtz departs wholly, as is well known, from Joh. Müller, and erects an imposing structure on quite a different plan. Müller had made the perception of the third dimension a matter of empirical judgment, but he had not determined the necessary moments of the process precisely. It was probably the investigations of Wheatstone in single vision with non-identical points and the observations of single vision in certain squint-eyed persons, together with Lotze's ingenious disquisition upon the space-problem, which gave rise to the first tendency in this direction. Helmholtz opposes the empirical to the nativist theory. Even those who are of the opinion that no one can be wholly just to the established psychological facts without certain concessions to the theory of nativism will, nevertheless, still most readily acknowledge the credit which Helmholtz has won for himself in his treatment of this cardinal question. The antithesis between nativism and empiricism has still more extended bearings; it appears in nearly every psychological question; it characterizes entire schools which have been opposed to one

another, especially in English psychology, for a long time. The exhaustive investigation of the materials for the theory of space from an empirical standpoint has greatly lightened the critical examination of the same for all subsequent thinkers. Always in the habit of searching for certain and clear criteria, Helmholtz sought some characteristic mark to distinguish between that which is a real sensation and that which arises from some supplementary experience. That is, he recognizes as sensation only that part of our intuition which can not be accounted for through assignable experience processes, which might have produced a contrary result.

Another principle of great value, also, to acoustics, concerns the discrimination of simultaneous sensations. We have been accustomed to regard a total of sensations constantly presented together as a common sign for a single object, and the discrimination of these sensations is thereby rendered more difficult. This principle he applied especially to the explanation of experiences of single-vision. The *Physiologische Optik*, as is well known, contains also his philosophical theory of the relation of our consciousness to the outer world, to which Helmholtz in his *Thatsachen der Wahrnehmung* latterly returned, the theory that the *a priori* self-evident law of causation necessitates the acceptation by us of an outer world; that, however, our knowledge of this outer world must remain essentially a symbolical one, and that our sensations are merely signs of what is real. On the occasion of his 'Jubilee,' Helmholtz spoke in quite an elegiac strain of the reception which these views had received at the hands of philosophers. Without inquiring to what extent the blame may be due to certain weaknesses of his representation, or to what extent, also, to the high plane of discussion and the subtilty of his general concepts, or on the other hand to the dearth of clear thinking on the part of some at least of the philosophers who have not passed through the exacting discipline of physics, we are, however, certain that all his philosophical colleagues felt extremely thankful to him for his gifts in this direction also. In my own personal opinion, at least, the theory of the

symbolical knowledge of the outer world really hits upon the right conception. But be that as it may, at any rate the introduction of such considerations into his work, and the accompanying evidence of the deepest interest in philosophical problems, has been largely instrumental in keeping alive and stimulating such interest among students of nature in all lands.

The number of new investigations which were published after the appearance of the *Physiologische Optik*, and called forth by that work, made a new edition of the book desirable long ago. The author had, however, in the meantime turned to other spheres of investigation, as that of electro-dynamics, and it was not astonishing that he decided only lately upon it, when it was no longer possible for him to give due attention to the large amount of material at hand. The successive parts of this work followed each other at ever lengthening intervals of time. The serious misfortune which befel the aged investigator last autumn upon his homeward journey from America urged him moreover to a careful husbanding of his strength, and so made it possible that he should allow the last portion of the work, which includes the discussion of contrast-phenomena, to appear with no reference to Hering's labors in this sphere. This will always be most deeply regretted. The half of the work which still remains unpublished will have to be simply reprinted, only perhaps with a revision of the bibliographical references.

The second psycho-physical work, *Lehre von den Tonempfindungen*, was published in 1863, during one of the intervals in the publication of the several parts of his first work. Studies extending through many years had preceded also in this field, some of which were already published, *Ueber Klangfarbe der Vocale*, *Ueber Combinationstöne*, etc., also the popular lecture upon '*Physiological Causes of Musical Harmony*,' 1857, in which the outlines of the whole work were sketched. One can hardly picture the immense intellectual labor which the carrying out of two such literary projects simultaneously must have involved. However, the problems in the theory of sensation were reciprocally illumi-

nated, and it was precisely the similarity of the phenomena in the two spheres which led Helmholtz to the common view-point already mentioned above. It was during the attractive period of the days of Bonn and Heidelberg that, as he himself afterwards related, while leisurely tramping over the wooded hills, these ideas flowed in upon his mind.

Indeed this second work seems, externally in the form of its presentation, to betray the influence of such a happy mood and friendly environment. It not only treats of the fundamental principles of art, but is itself truly a work of art, and that, too, without any flowers of rhetoric. It is confined to the simple unfolding of the absolutely essential features of the subject. His mightiest weapon, the mathematical calculations and the deductions from previously published computations, was relegated to the Appendix. In the text itself only the manifest principles of the theory are presented to the reader; he is led step by step from the simplest truths of physical acoustics to their physiological conditions, until finally, deep in the center of the system, he is brought to the consideration of the musical scales and harmony, and over the threshold into the æsthetics of music. Here the author deliberately stops.

In Acoustics, too, the achievements of Helmholtz were not creations out of nothing. The illustrious labors of others preceded them—especially Fourier's demonstration of the possibility of resolving any periodic vibration into a number of simple harmonic vibrations; Ohm's definition of simple tones by means of harmonic vibrations, and his correspondence with Seebeck (in which Ohm approached very near to the correct explanation of timbre); Seebeck's theory of sympathetic vibrations; Wilhelm Weber's investigations concerning reed-pipes; Joh. Müller's experimental studies upon vibrating membranes and the human voice; Chladni's caretaking observations, and finally on the side of the theory of music, Rameau's application of the overtone to the theory of harmony. That the application of the theory of beats in the preceding century for a similar purpose had already been attempted, was probably not known

to Helmholtz. But he was familiar with the the hypothesis of an isolation of tones in the cochlea held by many physiologists of the preceding decade, and Harless in Wagner's Dictionary had in fact treated the organ of Corti accordingly. There is, however, a vast difference between the expression of a thought in general terms and the presenting of it in its concrete setting or in its connection with a mass of facts, and it is a long way from eminent labors along single lines and from a one-sided standpoint, to the comprehensive, all around investigation of this entire field, which inspected all sides of the problem impartially, and went even into the depths of the history of music. The work has passed through four editions. Owing to the comparatively small number of subsequent works, as well as to the almost universal acquiescence in his opinions, it was possible to satisfy the demand for new editions without too great difficulty. The most essential changes concerned the significance of the organ of Corti (from the first to the second edition) and the theory of the psychological conditions of tone-analysis (from the third to the fourth edition). It seems almost a pity to have to acknowledge that even this magnificently constructed work will not in all its main features defy the ravages of time. This is, however, my firm conviction. The explanation of timbre, that old riddle, will, however, remain a permanent acquisition. But whether it was correct to deduce timbre and consonance from one and the same principle, that of the overtones, and to define dissonance by means of beats, that is the chief question.

What human achievement, however, can defy time and know no change? Considerations of a critical nature will not disturb the satisfaction with which we feel that our century has beheld an immense advance of science, and that we ourselves were contemporaries of the man who accomplished it. Only in the last few months has it been the privilege of the writer of these lines to know him. Whoever has been thus fortunate will always hold in lively remembrance the massive head, the large, thoughtful eyes, the repose of manner, the modest and unpretentious bearing.

But we shall always retain as his most valuable legacy the inseparable union of scientific and philosophical research, and the profound conception of the mental life, grounded throughout upon actual facts, and for that very reason the loftier and more truly ideal.*

* Translated from the author's manuscript by Professor John Grier Hibben, Princeton College.

THE THEORY OF EMOTION.

(II.) The Significance of Emotions.

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In a preceding article[1] I endeavored to show that all the so-called expressions of emotion are to be accounted for not by reference to emotion, but by reference to movements having some use, either as direct survivals or as disturbances of teleological coördinations. I tried to show that, upon this basis, the various principles for explaining emotional attitudes may be reduced to certain obvious and typical *differentiæ* within the teleological movements. In the present paper I wish to reconsider the James-Lange, or discharge, theory of the nature of emotion from the standpoint thus gained; for if all emotions (considered as 'emotional seizures,' *Affect*[2] or 'feel,' as I may term it) are constituted by the reflexion of the teleological attitude, the motor and organic discharges, into consciousness, the same principle which explains the attitude must serve to analyze the emotion.

The fact, if it be a fact, that all 'emotional expression' is a phase of movements teleologically determined, and not a result of pre-existent emotion, is itself a strong argument for the discharge theory. I had occasion to point out in my previous article that the facts brought under the head of 'antithesis' and 'analogous stimuli' are absolutely unaccountable upon the central theory, and are matters of course upon the James theory. But this statement may be further generalized. If every emotional attitude is referred to useful acts, and if the emotion is *not* the reflex of such act, where does

[1] Psychological Review, Nov., 1894.

[2] See this Review, Sept., 1894, p. 523.

it come in, and what is its relation to the attitude? The first half of the hypothesis prevents its being the antecedent of the attitude; the latter half of the hypothesis precludes its being the consequent. If it is said that the emotion is a mere side issue of that central excitation (corresponding to the purpose) which issues in the muscular and organic changes, then we are entitled to ask, *a priori*, for some explanation of its unique appearance at this point, some sort of mechanical or teleological *causa essendi;* and, *a posteriori*, to point out that, as matter of fact, every one now supposes that his emotion, say of anger, does have *some* kind of direct relation to his movements—in fact, common usage compels us to speak of them as movements of anger. I think, then, that logic fairly demands either the surrender of the 'central' theory of emotion or else a refutation of the argument of the preceding paper, and a proof that emotional attitudes are to be explained by reference to emotion, and not by reference to acts.

More positively, this reference to serviceable movement in explanation of emotional attitudes, taken in connection with the hypothesis that the emotional 'feel' is always due to the return wave of this attitude, supplies a positive tool for the analysis of emotion in general and of particular emotions in especial. As indicating the need of a further consideration, it may be pointed out that Mr. James himself lays the main emphasis of his theory upon its ability to account for the *origin* of emotions, and as supplying emotion with a 'physical basis,' not upon the psychological analysis which it might yield of the nature of emotional experience. Indeed, James definitely relegates to the background the question of classification,[1] saying that the question of genesis becomes all-important. But every theory of genesis must become a method of analysis and classification. The discharge theory does, indeed, give the *coup de grâce* to the fixed pigeon-hole method of classification, but it opens the door for the genetic classification. In other words, it does for the emotions precisely what the theory of evolution does in biology; it destroys the arbitrary and subjective schemes, based on mere

[1] *Psychology*, Vol. II., p. 454 and p. 485.

possession of likenesses and differences, and points to an objective and dynamic classification based on descent from a given functional activity, gradually differentiated according to the demands of the situation. The general conclusion indicated regarding the nature of emotion is that:

Emotion in its entirety is a mode of behavior which is purposive, or has an intellectual content, and which also reflects itself into feeling or Affects, as the subjective valuation of that which is objectively expressed in the idea or purpose.[1]

This formula, however, is no more than a putting together of James' theory with the revision of Darwin's principles attempted in the last number. If an attitude (of emotion) is the recurrence, in modified form, of some teleological movement, and if the specific differentia of emotional consciousness is the resonance of such attitude, then emotional excitation is the felt process of realization of ideas. The chief interest lies in making this formula more specific.

In the first place, this mode of getting at it relieves Mr. James's statement of the admittedly paradoxical air which has surrounded it. I can but think that Mr. James' critics have largely made their own difficulties, even on the basis of his 'slap-dash' statement that "we feel sorry because we cry, angry because we strike, afraid because we tremble." The very statement brings out the idea of *feeling* sorry, not of *being* sorry. On p. 452 (Vol. II) he expressly refers to his task as "subtracting certain *elements of feeling* from an emotional state supposed to exist *in its fulness*" (italics mine). And in his article in this REVIEW (Sept., 1894), he definitely states that he is speaking of an *Affect*, or emotional seizure. By this I understand him to mean that he is not dealing with

[1] In my *Psychology*, *e.g.*, p. 19 and pp. 246–249, it is laid down, quite schematically, that feeling is the internalizing of activity or will. There is nothing novel in the doctrine; in a way it goes back to Plato and Aristotle. But what first fixed my especial attention, I believe, upon James' doctrine of emotion was that it furnishes this old idealistic conception of feeling, hitherto blank and unmediated, with a medium of translation into the terms of concrete phenomena. I mention this bit of personal history simply as an offset to those writers who have found Mr. James' conception so tainted with materialism. On the historical side, it may be worth noting that a crude anticipation of James' theory is found in Hegel's Philosophie des Geistes, § 401.

emotion as a concrete whole of experience, but with an abstraction from the actual emotion of that element which gives it its differentia—its feeling *quale*, its 'feel.' As I understand it, he did not conceive himself as dealing with that state which we term 'being angry,' but rather with the peculiar 'feel' which any one has when he is angry, an element which may be intellectually abstracted, but certainly has no existence by itself, or as full-fledged emotion-experience.

What misled Mr. James' critics, I think, was not so much his language, as it was the absence of all attempts on his part to connect the emotional seizure with the other phases of the concrete emotion-experience. What the whole condition of *being* angry, or hopeful or sorry may be, Mr. James nowhere says, nor does he indicate why or how the 'feel' of anger is related to them. Hence the inference either that he is considering the whole emotion-experience in an inadequate way, or else—as Mr. Irons took it—that he is denying the very existence of emotion, reducing it to mere consciousness of bodily change as such. Certainly, even when we have admitted that the emotional differentia, or 'feel', is the reverberation of organic changes following upon the motor response to stimulus, we have still to *place* this 'feel' with reference to the other phases of the concrete emotion-experience. 'Common sense' and psychological sense revolt at the supposed implication that the emotional 'feel' which constitutes so much of the meaning of our lives is a chance arrival, or a chance super-imposition from certain organic changes which happen to be going on. It is this apparently arbitrary isolation which offends.

If, preparatory to attempting such a placing, we put before us the whole concrete emotional experience, we find, I think, that it has two phases beside that of *Affect*, or seizure. (1) It is a disposition, a mode of conduct, a way of behaving. Indeed, it is this practical aspect of emotion which common speech mainly means to refer to in its emotional terms. When we say that John Smith is very resentful at the treatment he has received, or is hopeful of success in business, or regrets that he accepted a nomination for office, we do not simply, or even chiefly, mean that he has a certain 'feel'

occupying his consciousness. We mean he is in a certain practical attitude, has assumed a readiness to act in certain ways. I should not fear a man who had simply the 'feel' of anger, nor should I sympathize with one having simply the 'feel' of grief.[1] Grief means *unwillingness* to resume the normal occupation, practical discouragement, breaking-up of the normal reactions, etc., etc. Just as anger means a tendency to explode in a sudden attack, not a mere state of feeling. We certainly do not deny nor overlook the 'feel' phase, but in ordinary speech the behavior side of emotion is, I think, always uppermost in consciousness. The connotation of emotion is primarily ethical, only secondarily psychical. Hence our insulted feeling when told (as we hastily read it—our interpretation is 'slap-dash' rather than the sentence itself) that we are not angry until we strike, for the sudden readiness to injure another is precisely what we mean by anger. Let the statement read that we do not have the emotional seizure, the 'feel' of anger, till we strike, or clench our fist, or have our blood boil, &c., and the statement not only loses its insultingly paradoxical quality, but (unless my introspection meets a different scene from that of others) is verified by every passing emotion. (2) But the full emotional experience also always has its 'object' or intellectual content. The emotion is always 'about' or 'toward' something; it is 'at' or 'on account of' something, and this prepositional reference is an integral phase of the single pulse of emotion; for emotion, as well as the idea, comes as a whole carrying its distinctions of value within it. The child who ceases to be angry *at* something—were it only the floor at last—but who keeps up his kicking and screaming, has passed over into sheer spasm. It is then no more an emotion of anger than it is one of æsthetic appreciation. Dis-

[1] I take it that this separation of 'feel' from practical attitude is precisely what makes the difference between an emotional and a sentimental experience. The fact that the 'feel' may be largely, though never wholly, simulated, by arousing certain organic excitations apart from the normal practical readiness to behave in a certain way, has played a sufficiently large part in our 'evangelical' religions. The depth, in a way, and the hollowness, in another way, of the subjectively induced religious sentiments seems to me, in itself, a most admirable illustration of the truth of James' main contention.

gust, terror, gratitude, sulkiness, curiosity—take all the emotions seriatim and see what they would be without the intrinsic reference to idea or object. Even the pathological or objectless emotion is so only to the rational spectator. To the experiencer (if I may venture the term) it subsumes at once its own object as source or aim. This feeling of depression must have its reason; the world is dark and gloomy; no one understands me; I have a dread disease; I have committed the unpardonable sin. This feeling of buoyancy must have its ideal reference; I am a delightful person, or one of the elect or have had a million dollars left me.[1]

It is perhaps at this point that the need of some reconstruction which will enable us to place the phases of an entire emotional experience becomes most urgent. In Mr. James' statement the experience is apparently (apparently, I say; I do not know how much is due to the exigency of discussion which necessitates a seeming isolation) split up into three separate parts: First comes the object or idea which operates only as stimulus; secondly, the mode of behavior taken as discharge of this stimulus; third, the *Affect*, or emotional excitation, as the repercussion of this discharge. No such seriality or separation attaches to the emotion as an experience. Nor does reflective analysis seem to establish this order as the best expression of the emotion as an object of psychological abstraction. We might almost infer from the way Mr. James leaves it that he is here a believer in that atomic or mosaic composition of consciousness which he has so effectively dealt with in the case of intellectual consciousness. However this may be, Mr. James certainly supplies us, in the underlying *motif* of this 'chapter' on emotion, with an adequate instrument of reconstruction. This is the thought that the organic discharge is an *instinctive* reaction, not a response to an idea as such.

Following the lead of this idea, we are easily brought to the conclusion that *the mode of behavior is the primary thing, and that the idea and the emotional excitation are constituted at one and the same time; that, indeed, they represent the tension*

[1] I do not mean, of course, that every 'pathological' emotion creates an intellectual delusion; but it does carry with it a changed intellectual coloring, a different direction of attention.

of stimulus and response within the coördination which makes up the mode of behavior.

It is sheer reflective interpretation to say that the activity in anger is set up by the object, if we by object mean something consciously apprehended as object. This interpretation, if we force it beyond a mere way of speaking into the facts themselves, becomes a case of the psychological fallacy. If my bodily changes of beating heart, trembling and running legs, sinking in stomach, looseness of bowels, etc., follow from and grow out of the conscious recognition, *qua conscious recognition*, of a bear, then I see no way for it but that the bear is already a bear of which we are afraid—our idea must be of the bear as a fearful object. But if (as Mr James' fundamental idea would imply, however his language may read at times) this reaction is not to the bear as *object*, nor to the *idea* of bear, but simply expresses an instinctive coördination of two organic tendencies, then the case is quite different. It is not the idea of the bear, or the bear as object, but a certain *act of seeing*, which by habit, whether inherited or acquired, sets up other acts. It is the kind of *coördination of acts* which, brought to sensational consciousness, constitutes the bear a fearful or a laughable or an indifferent object. The following sentence, for example, from James (this REVIEW, Vol. I. p. 518) seems to involve a mixture of his own theory with the one which he is engaged in combatting: "Whatever be our reaction on the situation, in the last resort it is an *instinctive reaction* on that one of its elements which strikes us for the time being *as most vitally important*." The conception of an instinctive reaction is the relevant idea; that of reaction upon an element 'which strikes us as important' the incongruous idea. Does it strike us, *prior* to the reaction, as important? Then, most certainly, it already has emotional worth; the situation is already delightful and to be perpetuated, or terrible and to be fled, or whatever. What does recognition of importance mean aside from the ascription of worth, value—that is, aside from the projection of emotional experience?[1] But I do not think

[1] It seems to me that the application of James' theory of emotion to his theory of attention would give some very interesting results. As it now stands, the theory

James' expression in this and other similar passages is to be taken literally. The reaction is not made on the basis of the apprehension of some quality in the object; it is made on the basis of an organized habit, of an organized coördination of activities, one of which instinctively stimulates the other. The outcome of this coördination of activities constitutes, for the first time, the object with such and such an import—terrible, delightful, etc.—or constitutes an emotion referring to such and such an object. For, we must insist once more, the frightful object and the emotion of fear are two names for the same experience.

Here, then, is our point of departure in placing the 'feel,' the 'idea,' and the 'mode of behavior' in relation to one another. The idea or object which precedes and stimulates the bodily discharge is in no sense the idea or object (the intellectual content, the 'at' or 'on account of') of the emotion itself. The particular idea, the specific quality or object to which the seizure attaches, is just as much due to the discharge as is the seizure itself. More accurately and definitely, the idea or the object is an abstraction from the activity just as much as is the 'feel' or seizure. We have certain organic activities initiated, say in the eye, stimulating, through organized paths of association in the brain, certain activities of hands, legs, etc., and (through the coördination of these motor activities with the vegetative functions necessary to maintain them) of lungs, heart, vaso-motor system, digestive organs, etc. The 'bear' is, psychologically, just as much a discrimination of certain values, within this total pulse or coördination of action, as is the feeling of 'fear.' The 'bear' is constituted by the excitations of eye and coördinated touch centres, just as the 'terror' is by the disturbances of muscular and glandular systems. The reality, the coördination of these partial activities, is that whole activity which may be described equally well as 'that terrible bear,' or 'Oh, how frightened I am.' It is precisely

'in attention' of preferential selection on the *basis* of interest seems to contradict the theory of emotional value as the *outcome* of preferential selection (that is, specific reaction). But the contradiction is most flagrant in the case of effort, considered, first, as emotion and then as an operation of will.

and identically the same actual concrete experience; and the 'bear,' considered as one experience, and the 'fright,' considered as another, are distinctions introduced in reflection upon this experience, not separate experience. It is the psychological fallacy again if the differences which result from the reflection are carried over into the experience itself. If the fright comes, then the bear is not the bear of that particular experience, is not the object to which the feeling attaches, *except* as the fright comes. Any other supposition is to confuse the abstract bear of science with the concrete (*just this*) bear of experience.

The point may be further illustrated by the objection which Mr. Irons has brought against the James theory. (Mind, 1894, p. 85). "How can one perceptive process of itself suffuse with emotional warmth the cold intellectuality of another?" Note here the assumption of two distinct 'processes', apparently recognizing themselves as distinct, or anyhow somehow marked out as different in themselves. The continued point of Mr. Irons' objection is that Mr. James makes intellectual and emotional 'states', (values) the knowledge of an object and the emotion referred to it, both due to currents from the periphery, and the same kind of current cannot be supposed to induce such radically different things as an intellectual and an emotional process. The objection entirely overlooks the fact that we have but the one organic pulse, the frightful bear, the frightened man, whose reality is the whole concrete coördination of eye--leg—heart, &c., activity, and that the distinction of cold intellectuality and warm emotionality is simply a *functional* distinction within this one whole of action. We take a certain phase which *serves a certain end*, namely, giving us information, and call that intellectual; we take another phase, having another end or value, that of excitement, and call that emotional. But does any one suppose that, *apart from our interpretation of values*, there is one process in itself intellectual, and another process in itself emotional? I cannot even frame an idea of what is meant. I can see that the eye-touch process gives us information mainly, and so we call that intellectual; and that the heart-bowels process gives us the valuation of this informa-

tion in terms of our own inner welfare,—but aside from this distinction of ~~values within~~ a concrete whole, through reflection upon it, I can see nothing.

If, then, I may ~~paraphrase~~ Mr. James' phraseology, the statement would read as follows: Our customary analysis, reading over into the experience itself what we find by interpreting it,[1] says we have an idea of the bear as something to be escaped, and so run away. The hypothesis here propounded is that the factors of a coördination (whether due to inherited instinct or to individually acquired habit) begin to operate and we run away; running away, we get the idea of 'running-away-from-bear', or of 'bear-as-thing-to-be-run-from.' I suppose every one would admit that the complete, mature idea came only in and through the act of running, but might hold that an embryonic suggestion of running came before the running. I cannot disprove this position, but everything seems to point the other way. It is more natural to suppose that as the full idea of running away comes in from the full execution, so the vague suggestion comes through the vague starting-up-of the system, mediated by discharge from the centres.

The idea of running away must certainly involve, as part of its content, an excitation of the 'motor-centres' actually concerned in running; it would seem as if this excitation must involve some, however slight, innervation of the peripheral apparatus involved in the act.[2] What ground is there for supposing that the idea comes to consciousness save through the sensorial return of this peripheral excitation? Is there any conceivable statement, either in terms of introspection or of nervous structure, of an idea of movement coming to consciousness absolutely unmediated peripherally? Sensorial consciousness, mediated by the incoming

[1] This is simply circumlocution for 'common-sense.' Common-sense is practical, and when we are practical it is the value of our experience, what we can get out of it or think we can, that appeals to us. The last thing that concerns us is the actual process of experiencing, *qua* process. It might almost be said that the sole difficulty in psychology, upon the introspective side, is to avoid this substitution of a practical interpretation of an experience for the experience itself.

[2] I do not mean that this innervation comes to consciousness as such; on the contrary.

current, is an undoubted fact; it is *vera causa*. Putting the two hypotheses side by side simply as hypotheses, surely the logical advantage of economy and of appeal to *vera causa* is on the side of the theory which conceives the idea of movement in terms of a return of discharge wave, and against that which would make it a purely central affair.[1]

But this is far from being all. I suppose one is fairly entitled now to start from the assumption of a sensory-continuum, the 'big, buzzing, blooming confusion,' out of which particular sensory quales are differentiated. Discrimination, not integration, is the real problem. In a general way we all admit that it is through attention that the distinctions arise, through selective emphasis. Now we may not only rely upon the growing feeling that attention is somehow bound up with motor adjustment and reaction, but we can point to the specific facts of sensorial discrimination which show, that, as a matter of fact, the range and fineness of discrimination run parallel to the apparatus for motor adjustments. We can also show that, in the only case in which there has, as yet, been a serious attempt to work out the details of discrimination, namely, space distinctions, all hands agree that they come through motor adjustments—the question whether 'muscular' or joint surface sensations are primary, having here no importance. Such being the case, how can the particular stimulus which excites the discharge be defined as *this* or *that* object apart from our reaction to it? I do not care to go into the metaphysics of objective qualities, but dealing simply with the psychological recognition of such qualities, what basis or standard for qualitative definiteness can we have, save the consciousness of differences in our own organic response? The bear may be a

[1] There are further logical grounds for expecting acquiescence from those who accept the general standpoint of Mr. James. To say nothing of the insistence upon consciousness as essentially reactive or motor, 'idea' and emotional seizure hang together. Fear-of-bear, bear-as-fearful-object cannot be separated. Besides, when I introspect for my 'fringe' in the stream of thought I always find its particular sensorial basis in shiftings of directions and quantity of breath, and other slight adjustments, just as certainly as I always can pick out the sensorial basis for my emotional seizures. *A priori*, it is difficult to see what the 'fringe' can be save the feeling of the running accompaniment of aborted acts, having their value now only as signs or cues, but originally complete in themselves.

thousand times an individual entity or distinct object metaphysically, if you please; you may even suppose, if you will, that the particular wave-lengths which deflect from the bear, somehow sort themselves out from the wave-lengths coming from all the rest of the environment, and come to the brain as a distinct bundle or package by themselves—but the recognition of just *this* object out of the multitude of possible objects, of just this bundle of vibrations out of all the other bundles, still remains to be accounted for. The predominating motor response supplies the conditions for its objectification, or selection. There is no competing hypothesis of any other machinery even in the field.

We return, then, confirmed, to our belief that the mode of behavior, or coördination of activities, constitutes the ideal content of emotion just as much as it does the *Affect* or 'feel', and that the distinction of these two is not given in the experience itself, but simply in reflection upon the experience. The mode of action constituted by the organic coordination of certain sensori-motor (or ideo-motor) activities, on one side, and of certain vegetative-motor activities on the other, is the reality, and this reality has a value, which, when interpreted, we call intellectual, and a value which, when interpreted we call Affect, or 'feel'. In the terms of our illustration, the mode of behavior carried with it the concept of the bear as a thing to be acted towards in a certain way, and of the 'feel' of our reaction. It is brown and chained—a 'beautiful' object to be looked at. It is soft and fluffy—an 'æsthetic' object to be felt of. It is tame and clumsy—an 'amusing' object to while away time with. It is hungry and angry—and is a 'ferocious' object to be fled. The consciousness of our mode of behavior as affording data for other possible actions constitutes the bear an objective or ideal content. The consciousness of the mode of behavior as something in itself—the looking, petting, running, etc.—constitutes the emotional seizure. In all concrete experience of emotion these two phases are organically united in a single pulse of consciousness.

It follows from this that all emotion, as excitation, involves inhibition. This is not absolute inhibition; it is not

suppression or displacement. It is incidental to the coördination. The two factors of the coördination, the 'exciting stimulus' and the excited response, have to be adjusted, and the period of adjustment required to affect the coördination, marks the inhibition of each required to effect its reconstruction as an integral part of the whole act. Or, since we have recognized that the exciting stimulus does not exist as fact, or object, until constituted such by the coördination in the final act, let us say that the activities needing adjustment, and so partial inhibition, are the kinæsthetic (sensori-motor or ideo-motor) activities which translate themselves into the 'object', and the vegetative-motor activities which constitute the 'reaction' or 'response' to the 'object'.

But here, again, in order to avoid getting on the wrong track it must be noted that this distinction of 'object' and 'response' is one of interpretation, or value, and not a plain matter of course difference in the experiencing. I have already tried to show that the 'object' itself is an organic excitation on the sensori-motor, or, mediately, ideo-motor side, and that it is not *the* peculiar object *of* the emotion until the mode of behavior sets in, and the diffusive wave repercussates in consciousness. But it is equally necessary to recognize that the very distinction between exciting or stimulating sensori-motor activity and excited or responding vegetative-motor activity is teleological and not merely factual. It is because these two activities have to be coördinated in a single act, to accomplish a single end, and have therefore to be so adjusted as to coöperate with each other, that they present themselves as stimulus and response. When we consider one activity, say the sensori-ideo-motor activity, which constructs or constitutes the bear as an 'object', not in itself, but from the standpoint of the final act into which it merges—the stopping to look at the bear and study it scientifically, or enjoy its clumsy movements—that activity takes the form of stimulus. So the vegetative-motor activity, which is, in itself as direct experience, simply the intrinsic organic continuation of the sensori-motor activity, being interpreted again as a reduced factor of, or contribution to, the final outcome, assumes the form of response.

But, I repeat, this distinction of stimulus and response is one of interpretation, and of interpretation from the standpoint of the value of some act considered as an accomplished end.

The positive truth is that the prior and the succeeding parts of an activity are in operation together; that the prior activity beside passing over into the succeeding also persists by itself, and yet that the necessary act cannot be performed until these two activities reinforce each other, or become contributing factors to a unified deed. The period of maximum emotional seizure corresponds to this period of adjustment. If we look at the deflection or reconstruction which either side undergoes during this adjustment, we shall call it inhibition—it is arrest of discharge which the activity would perform, if existing by itself. If we look at the final outcome, the completed adjustment, we have coördination.

I think it must be obvious that this account in no way runs athwart Mr. James' denial of inhibition as a necessary phase of the *Affect* (Psychology, Vol. II., p. 476, note). He there speaks of inhibition as if it could mean only complete suppression—which is no inhibition at all, psychologically, since with suppression or displacement, all tension vanishes. It is, indeed, a question of primary impulsive tendencies, but of these tendencies as conflicting with one another and therefore mutually checking, at least temporarily, one another. Acts, which in past times, have been *complete* activities, now present themselves as contemporaneous phases of one activity. In so far as they were once each complete in itself, there is struggle of each to absorb or negate the other. This must either occur or else there is a readjustment and a new whole, or coördination, appears, they now being contributory factors. The inhibition once worked out, whether by displacement of one or by reconstruction of both contending factors, the *Affect* dies out.

This sort of inhibition the James theory not only permits, but demands—otherwise the whole relation between the exciting stimulus and the instinctive response, which is the nerve of the theory, disappears. If the exciting stimulus does not persist over into the excited response, we get simply a case of habit. The familiar fact that emotion as excite-

ment disappears with definiteness of habit simply means that in so far as one activity serves *simply* as means, or cue, to another and gives way at once to it, there is no basis for conflict and for inhibition. But if the stimulating and the induced activities need to be coördinated together, if they are both means contributing to one and the same end, then the conditions for mere habit are denied, and some struggle, with incidental inhibitory deflection of the immediate activity, sets in. In psychological terms, this tension is always between the activity which constitutes, when interpreted, the object as an intellectual content, and that which constitutes the response or mode of dealing with it. There is the one phase of organic activity which constitutes the bear as object; there is the other which would attack it, or run away from it, or stand one's ground before it. If these two coördinate *without friction*, or if one immediately displaces the other, there is no emotional seizure. If they coexist, both pulling apart as complete in themselves and pulling together as parts of a new whole, there is great emotional excitement.[1] It is this tension which makes it impossible to describe any emotion whatever without using dual terms—one for the *Affect* itself, the other for the object 'at', 'towards,' or 'on account of,' which it is.

We may now connect this analysis with the result of the consideration of the emotional attitudes. The attitude is precisely that which was a complete activity once, but is no longer so. The activity of seizing prey or attacking an enemy, a movement having its meaning in itself, is now reduced or aborted; it is an attitude simply. As an instinctive reaction it is thoroughly ingrained in the system; it represents the actual coördinations of thousands and thousands of

[1] See James, II., 496–497. But more particularly I should apply to the difference between relatively indifferent and emotionally excited consciousness precisely what James says of the difference between habitual and reasoned thinking. (II., p. 366.) "In the former, an entire system of cells vibrating at any one moment discharges in its totality into another system, the order of the discharges tends to be a constant one in time; whilst in the latter a part of the prior system still keeps vibrating in the midst of the subsequent system, and the order . . . has little tendency to fixedness in time." Add to this that it is necessary to perform a unified act—or reconstitute a single, comprehensive system, and the reality (though strictly incidental character) of inhibition appears.

ancestors; it tends to start into action, therefore, whenever its associated stimulus occurs. But the very fact that it is now reduced to an attitude or tendency, the very fact that it is now *relatively* easy to learn to control the instinctive blind reaction when we are stimulated in a certain way, shows that the primary activity is inhibited; it no longer exists as a whole by itself, but simply as a coördinated phase, or a contributory means, in a larger activity. There is no reason to suppose that the original activity of attack or seizure was emotional, or had any *quale* attached to it such as we now term 'anger'. The animal or our ancestor so far as it was given up without restraint to the full activity undoubtedly had a feeling of activity; but just because the activity was undivided, it was not 'emotion'; it was not 'at', or 'towards' an object held in tension against itself. This division could come in only when there was a need of coördinating the activity which corresponded to the perception and that which corresponded to the fighting, as means to an activity which was neither perceiving nor fighting. The animal growling and lashing its tail as it *waits* to fight may have an emotional consciousness, but even here, there may be, for all we know, simply a unified consciousness, a complete concentration on the act of maintaining that posture, the act of waiting being the *adequate* response to the given stimulus. Certainly,[1] so far as I can trust my own introspection, whenever my anger or any strong emotion has

[1] I have no intention here of constructing, *a priori*, the animal consciousness. I use this merely as hypothetical illustration; *if* unification of activity, then no emotion; *if* emotion, then tension of intellectual recognition on one side and consideration of how to behave towards object recognized on the other. I must add, however, that such interpretations as Darwin's umbrella case (in his *Descent of Man*), as illustrating a rude sense of the supernatural, seem to me most unwarrantably anthropomorphic. Surely, the only straightforward interpretation is, there was interruption of a reaction which had started to discharge, and that such a change in stimulus suddenly set up another discharge totally at cross-purposes with the first, thus disintegrating the animal's coördinations for a moment. Unless the animal recognizes or objectifies the familiar reaction, and recognizes also the unexpected reaction in such a way that there tension arises between the two, there can be no *emotion* in the animal, but simply a shock of interrupted activity—the sort of fit which James speaks of, Vol. II, 420. It may well be that the feeling of the supernatural in man, however, *is* precisely the feeling of such tension—instead of there being an idea of the supernatural, and then an associated feeling of terror towards it.

gained complete possession of me, the peculiar *Affect quale* has disappeared. I remember well a youthful fight, with the emotions of irritation and anger before, and of partial fear and partial pride afterwards, but as to the intervening period of the fight nothing but a strangely vivid perception of the other boy's face as the hypnotizing focus of all my muscular activities. On the other side, my most intense and vengeful feelings of anger are associated with cases where my whole body was so sat on as to prevent the normal reaction. Every one knows how the smart and burn of the feeling of injustice increases with the feeling of impotency; it is, for example, when strikes are beginning to fail, that violence from anger or revenge, as distinct from sheer criminality, sets in. It is a common-place that the busy philanthropist has no occasion to feel the extreme emotion of pathos which the spectator or reader of literature feels. Cases might be multiplied ad libitum.

It is then in the reduction of activities once performed for their own sake, to attitudes now useful simply as supplying a contributory, a reinforcing or checking factor, in some more comprehensive activity, that we have all the conditions for high emotional disturbance. The tendency to large diffusive waves of discharge is present, and the inhibition of this outgoing activity through some perception or idea is also present. The need of somehow reaching an adjustment of these two sides is urgent. The attitude stands for a recapitulation of thousands of acts formerly done, ends formerly reached; the perception or idea stands for multitudes of acts which may be done, ends which may be acted upon. But the immediate and present need is to get this attitude of anger which reflects the former act of seizing into some connection with the act of getting-even or of moral control, or whatever the idea may be. The conflict and competition, with incidental inhibition and deflection, is the disturbance of the emotional seizure.

Upon this basis, the apparent strangeness or absurdity in the fact that a mere organic repercussation should have such tremendous values in consciousness disappears. This organic return of the discharge wave stands for the entire effort of

the organism to adjust its formed habits or coördinations of the past to present necessities as made known in perception or idea. The emotion is, *psychologically, the adjustment or tension of habit and ideal*, and the organic changes in the body are the literal working out, in concrete terms, of the struggle of adjustment. We may recall once more the three main phases presented in this adjustment as now giving us the basis of the classification of the emotions. There may be a failure to adjust the vegetative-motor function, the habit, to the sensori-(or ideo-) motor; there may be the effort, or there may be the success. The effort, moreover, also has a double form according as the attempt is in the main so to use the formed reactions as to avoid or exclude the idea or object, setting up another in its place, or to incorporate and assimilate it—*e. g.*, terror and anger, dread and hope, regret and complacency, etc.[1]

I shall not carry out this classification; but further suggest that, in my judgment, we now have the means for discriminating emotion as *Gefühlston*, as emotional disturbance, or *Affect* (with which we have been dealing so far) and as interest.

Interest is the feeling which arises with the completed coördination. Let the tension solve itself by successive displacements in time, *i. e.*, means assuming a purely serial form in which one stimulates the next, and we get the indifference of routine. But let the various means succeed in organizing themselves into a simultaneous comprehensive whole of action, and we have interest. All interest, *qua* interest, it would follow from this, is qualitatively alike, being differentiated simply by the idea to which it attaches. And expe-

[1] Because of the tension, however, these cannot be set over against each other absolutely. All terror, till it passes into pathological fright, involves anger, and anger some fear, etc. All moral experience is only too full of the subtle and deceiving ways in which regret (condemnation) and complacency (self-approbation) run into each other. There is the Pharisee who can maintain his sense of his own goodness only by tension with his thought of evil; or who can make his depth of remorse material for self-gratulation. And there is the sentimental selfish character which disguises its own disgrace from itself by emotional recognitions of the beauty of goodness, and of its own misfortunes in not being able, in the past, to satisfy this ideal. I have never known other such touching tributes to goodness as can proceed from the sentimental egoist, when he gets into 'trouble,' as he euphemistically terms it.

rience seems to verify this inference. Interest is undisturbed action, absorbing action, unified action, and all interests, *as interests*, are equally interesting. The collection of postage stamps is as absorbing, if it *is* absorbing or an interest, as the discovery of double-stars; and the figuring of indefinite columns of statistics as the discovery of the nature of sympathy. Nor is this a pathological principle, as it might seem to be were we to instance merely fads or hobbies. The multiplicity of deeds which demand doing in this world is too great to be numbered; that principle which secures that if only full or organic activity go into each end, each act shall equally satisfy in its time and place, is the highest ethical principle; it is the statement of the only religious emotional experience which really seems worth while—the sense of the validity of all necessary doing. I cannot dwell upon this matter of interest, but I suggest the case of purely scientific interest as crucial. On one side, it seems wholly unemotional, so free from all disturbance or excitation may it become; on the other, it represents a culmination of absorption, of concentrated attention. How this apparent paradox is to be dealt with save on the supposition that emotion (as *Affect*) is the feeling of tension in action, while interest is the feeling of a complex of relevant activity unified in a single channel of discharge, I do not see.

As for the *Gefühlston*, I shall only state the conclusion that would seem to follow from a thorough-going application of the principle already laid down. I do not know that this complete application is advisable, much less necessary, but I share somewhat in the feeling of Mr. Baldwin as expressed in the Nov. number (p. 617) of this REVIEW, that there is a presumption that a unitary principle holds all the way through.[1] At all events, those who have followed me so far may like to see how the hypothesis already propounded might conceivably apply to the case of, say, delight in certain tones,

[1] It hardly seems fair, though, to charge Mr. James with inconsistency because he declines to force his theory beyond the limits of the facts upon which he feels himself to have a sure hold. Surely we may admire this reserve, even if we cannot imitate it, instead of virtually accusing him of giving away his whole case by admitting, hypothetically, the existence of facts whose explanation would require an opposite principle.

colors or tastes, while those who do not accept the hypothesis will hardly be shocked at one absurdity the more.

The suggestion, then, is that the *Gefühlston* represents the complete consolidation of a large number of achieved ends into the organic habit or coördination. It is interest read backwards. That represents the complete identification of the habits with a certain end or aim. The tone of sense-feeling represents the reaction, the incorporate identification, of the successful ends into the working habit. It is not, as I have hitherto indicated, habit as habit which becomes feelingless; it is only the habit which serves as mere means, or serial stimulus. That a given coördination should assume into itself the value of all associated coördinations is a fact of every day experience. Our eye-consciousness takes up into itself the value of countless motor and touch experiences; our ear takes up the value of motor and visual experiences, &c. There is no apparent reason why this vicarious assumption should not become so organically registered—*pace* Weissman—as to become hereditary; and become more and more functionally incorporated into structure.

To sum up:—Certain movements, formerly useful in themselves, become reduced to tendencies to action, to attitudes. As such they serve, when instinctively aroused into action, as means for realizing ends. But so far as there is difficulty in adjusting the organic activity represented by the attitude with that which stands for the idea or end, there is temporary struggle and partial inhibition. This is reported as *Affect*, or emotional seizure. Let the coördination be effected in one act, instead of in a successive series of mutually exclusive stimuli, and we have interest. Let such coördinations become thoroughly habitual and hereditary, and we have *Gefühlston*.

END

THE MUSCULAR SENSE AND ITS LOCATION IN THE BRAIN CORTEX.

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It is not necessary to present to the readers of this journal any lengthy discussion in regard to the muscular sense. Every psychologist admits that there is a sense of movement which enables us to appreciate, (1) the position of a limb in space; (2) the degree and force of muscular action necessary to change the position of that limb; (3) the power needed to oppose varying resistances to the motion of that limb. It has been thought by some that the muscular sense was materially aided by the tactile sense in the process of accurate guiding and adjustment; it has been held by others that the muscular sense was wholly independent of the other senses and the fact here recorded supports this latter view. It has been held by some, that the centres of perception of the muscular sense in the cortex were identical with the centres of movement in the cortex; it has been held by others, that these centres were separate from one another; and the fact here recorded, supports this latter view.

It is well known that disturbance in muscular sense may be produced by diseases in various parts of the nervous system. Thus we have ataxia or incoördination of movement, not depending upon paralysis of the muscles, but entirely due to a lack of appreciation of muscular sense impressions, from (1) diseases of the peripheral nerves; (2) diseases of the posterior columns of the spinal cord, as shown in locomotor ataxia; (3) diseases of the lemniscus or its radiation in the internal capsule in its course toward the cortex around the Rolandic fissure; (4) general diffuse diseases of the cortex of the brain, such as general paresis. It is evident therefore,

that any defect in the tract conveying muscular sense from the muscles to the brain cortex, will produce a disturbance in the power of coördination.

Hitherto, facts have been wanting to determine the actual position of the termination of this tract in the cortex and the exact location of the muscular sense centres. The following observation, therefore, is one of considerable value, inasmuch as it illustrates the possibility of producing an entire loss of muscular sense by a limited destruction of the brain cortex, without producing at the same time, any disturbance in motor power or in tactile sensibility; and determines the localization of the muscular sense centre for the hand in the parietal region.

The case presents a set of facts quite analogous to those obtained in a physiological experiment and is one of considerable interest.

A young man was brought to the Presbyterian Hospital, suffering from intense headache, to the left of, and somewhat behind the vertex, and from epilepsy. He had been a healthy boy until his fifth year, when he had a severe fall on his head, which was followed by unconsciousness for several hours. Since that time he had never completely gained his mental balance. He had seemed fairly bright at his lessons, and willing to study, but was very easily agitated and accustomed to give way to emotional excitement or passion; his memory was good, but his powers of application somewhat deficient. When he was sixteen years old, he had another fall on his head followed by unconsciousness, and from that time his symptoms were all increased. The headache was very intense, quite constant, and subject to sudden periods of increase. When the pain increased exceedingly, the boy would develop a maniacal condition, in which his actions were extravagant, his speech abusive and profane, and in which he resorted to acts of violence toward his family and employers. These attacks occurred every few days, unless reduced in frequency by the use of bromide of potash; but in spite of treatment, would occur every three or four weeks. After the attack was over, he had no recollection of what had occurred during it, and on several occasions, he lost con-

sciousness during the attack and slept heavily after it; he never had any convulsions. This condition had been present for five years, when he came to the hospital. It was thought that the attacks were of the nature of epilepsy, being of the variety known as the epileptic psychical equivalent, in which condition a sudden attack of mental aberration takes the place of a convulsion, and though the patient is apparently conscious, he subsequently has no recollection whatever of his acts during an attack. The fact that these attacks had developed after a fall on the head, and that he suffered constantly from severe pain at the seat of the injury, led to an attempt to relieve the condition by opening the skull. Dr. McCosh, Surgeon to the Presbyterian Hospital, trephined him and found, upon exposing the brain, a small vascular tumor lying beneath the point of injury, directly upon the brain surface. The size of this vascular tumor was about ¾ of an inch in diameter. It was removed without producing any injury to the surface of the brain, so far as could be determined. The brain was explored by thrusting a needle into it in three directions, in view of the possibility of finding a small collection of fluid beneath the surface; but nothing was found. The boy recovered from the operation rapidly, so that within ten days he was quite well; but immediately after the operation it was found that he had lost his muscular sense in the right hand and arm, below the elbow. Attention was called to the fact by the peculiar awkwardness in the movement of hand and arm. Any attempt to grasp a pencil or glass of water or to pick up a pin, resulted in most excessive motions of an irregular type, without the possibility of carrying out the desired movement, even when guided by sight. The attempt to place his finger upon his nose with his eyes closed, failed; the finger being carried beyond the side of the head and above it; in fact all voluntary guidance of the hand was imperfect. At the same time his strength was as good as ever, his grip was greater in the right hand than in the left, so that the defect of movement was in no way due to an actual loss of power. When his eyes were closed he was absolutely unable to tell what position had been given to his fingers or hand

by the examiner; he did not know whether his hand was open or closed; when his hand and fingers were placed in a position and he was requested to put the other hand in the same position, his eyes being shut, he was totally unable to do so; he was unable to estimate with any degree of accuracy, substances different in weight in the right hand, though able to detect the differences readily with the left hand. It was evident that his awkwardness of movement was largely due to the inability to adjust his motions with the necessary degree of power. At the same time his tactile sense and sensation of temperature and pain were perfectly normal. There was no disturbance of any kind in the face or leg. This condition began to pass off about three weeks after the operation, and at the end of three months, he had recovered his muscular sense entirely. It was therefore evident, that this particular effect had been produced by a small localized injury of the cortex of the brain, which had been subsequently repaired by nature. The exact position of the cortex injured was easily determined, and it was found to be about two inches behind the fissure of Rolando and about an inch and a half to the left of the median line, at about the junction of the superior and inferior parietal lobules. This observation would therefore indicate: first, that the muscular sense centres are distinct in their location from tactile or pain or temperature sense centres; and also from the motor centres; secondly, that they are situated just behind the motor area in the parietal region of the brain.

A LOCATION REACTION APPARATUS.

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The problem which stimulated to the designing of this apparatus was that of testing the power of an individual to quickly and accurately touch an object suddenly disclosed to him in an unexpected position. In order to make the problem as simple as possible the apparatus (see Figure 1) was so devised that the subject is required to make a movement of the finger from the end of the nose to some portion of the arc of a circle of which he is the centre and whose plane is at the level of his elbow. Three positions were selected to give a wide range of movement, namely, the centre immediately in front and a point on each side at a distance of about 14 in. (A, B and C). The object to be touched consists of a white spot ½ in. in diameter, which may be placed at any one of these points without the knowledge of the subject, a screen being in front, arranged to fall at the proper time and instantly disclose the spot. In connection with this, a pendulum chronoscope is used which measures the interval of time between the falling of the screen and the touching of the white spot.

The apparatus for determining the error is constructed to measure the distance of the centre of the finger (Fig. 2, F) from the centre of the white spot (S) on either side, thus showing the error of the movement executed and its direction. It consists of a horizontal strip (St) of blackened brass 7 in. long, bearing in its centre the white spot (S). This is hinged along one side so that the finger pressure makes an electrical contact (E) to determine the end of the time interval and also releases the clamp controlling the error recording apparatus. Below this are two light arms (GG) pivoted

at a common point directly under the white spot, so that their tips project above the first strip about ¾ in. These arms are connected by a spring (Sp) tending to pull them together, but are held apart in the preliminary position by the

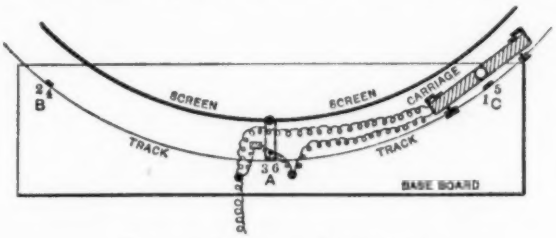

FIG. 1.—LOCATION APPARATUS.

pressure of the clamp projecting downward from strip (St, not shown in diagram) and are released by the touch of the subject, springing instantly to grasp the finger (F) between them. The raising of the finger clamps them anew in this position, and the displacement of the index showing the mid point of the finger can be read on its scale (R–L). This is found to work very quickly and conveniently with practically no observation error. A frame work carries the various parts and a set of wheels enables it to be run into any position desired.

The chronoscope has a balanced pendulum (Fig. 3), 12 inches total length, so weighted (W) that the time of swing is about a second and a half. The pendulum (P) carries a

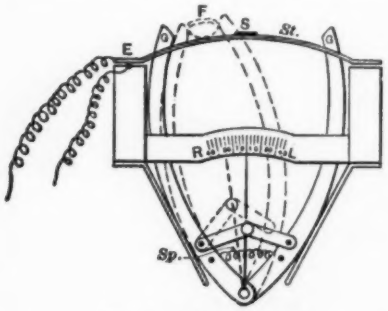

FIG. 2.—ERROR INDEX.

light index (I) that may be clamped instantly in any position on the scale (S), which latter was graduated empirically in hundredths of a second by a falling weight. The pendulum

is held in the preparatory position (Fig. 4) by means of a hook (H) connected with the armature (A) of an electromagnet

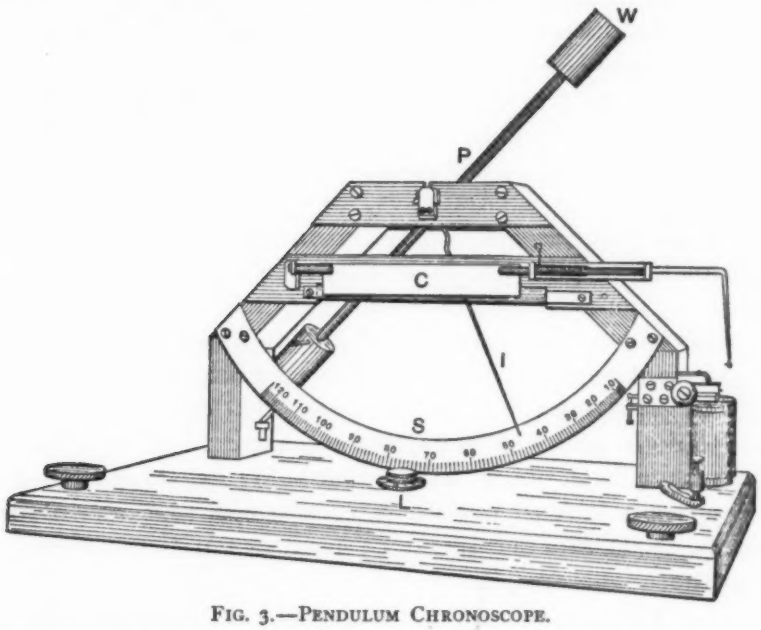

FIG. 3.—PENDULUM CHRONOSCOPE.

(M). The breaking of the circuit by the fall of the screen releases (Fig. 5, R) the pendulum carrying its index; the re-

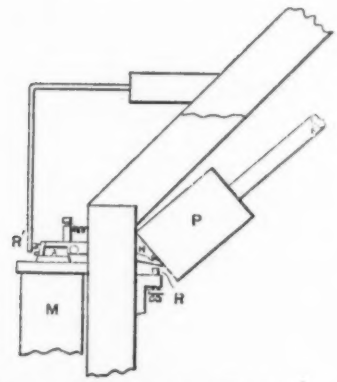

FIG. 4.—PENDULUM AND INDEX CLAMP RELEASE.

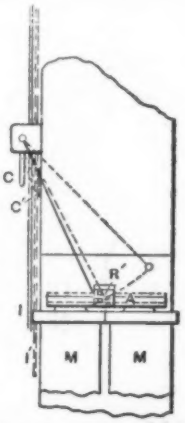

FIG. 5.—CLAMP RELEASE.

making of the circuit, by the touch of the subject's finger, releases (Fig. 5, R) the clamp (C) and catches the index so that

the time may be read upon the scale (Fig. 3, S). There is a level (L) upon the base board to enable one to put it in an exactly horizontal position, and the error of the instrument is thereby reduced to a negligible quantity. The details of the release are shown in Figures 4 and 5.

By means of this apparatus it was hoped to measure some of the elements making up the differences which exist between individuals in their power to do certain things requiring quickness and accuracy, as, for instance, tennis playing and fencing, the essential requirements being the perception and quick interpretation of external conditions, followed instantly by an appropriate motor response. The apparatus gives us somewhat similar conditions to those offered by the games mentioned, but gives them so definitely that it is possible to get a numerical statement of what each individual is able to do in terms of quickness and accuracy. The differences have been found to be remarkably great, and there is an apparent lack of coördination between time and error; that is, those who are quick are not necessarily less accurate than those who are slower.

The accompanying table shows the result of some work with the apparatus, and is given to suggest the wide range of individual ability thus tested.

Time in $\frac{1}{100}$ sec.	Males. No.	Males. % of total.	Males. Av. error.	Females. No.	Females. % of total.	Females. Av. error.
27– 35	11	6.2	11.1	1	1.5	10.
35– 45	48	22.5	10.05	12	18.	9.4
45– 55	54	31.	8.25	18	26.	7.8
55– 65	29	17.	9.	25	35.	7.2
65– 75	18	11.	8.2	11	16.	5.4
75– 85	8	5.	3.1	4	5.	4.4
85– 95	4	2.	4.05	0		
95–105	0	0.	0	1		
105–115	1	.6	7.8			

The tests were made in three positions (A, B, C, in the order 1, 2, 3, 4, 5, 6), as described, every individual making

four attempts in each position, twelve all told. These were recorded separately, hence it is possible to study each effort in relation to the position in which it was made. This was done for both hands to compare the right with the left in regard to quickness, accuracy and direction of error; but it has been thought best not to include a discussion of the results from this standpoint in the present paper.

The table contains a study of the observations made with the right, or preferred, hand by 173 males and 72 females, all those of one individual being treated here as if made in one position. They were obtained from several sources, a large portion of them being derived from the Psychological Laboratory of the Columbian Exposition, Chicago, where the apparatus was in use by Prof. Jastrow, and also from Harvard students and in the Harvard Summer School of Physical Training. Inasmuch as it did not seem possible to make a fair classification of these, they have been arranged in two divisions, male and female, regardless of ages and experiences.

The first column of the table gives the limits of quickness, determining each group of these two classes: the second, in the two divisions of the table, gives the number of individuals whose reactions lie between the limits noted: the third gives the percentage of this number to the total number in the class, while the fourth gives the average error which is a measure of the accuracy of the movements. It will be noticed that the number of individuals of the different groups shows a distinct distribution curve with the apex at about 0.5 Sec. in the males and 0.6 Sec. in the females, suggesting that these are near the means. Of course, this quickness is made up of the reaction—time proper and the time occupied in making the movement from the end of the nose to the plane of the apparatus. It will be noticed also that the average errors for these groups do not vary in ratio to the quickness, but that those who make the movement in .35 Sec. are almost as accurate as the group making the movement in .75 Sec., some being, indeed, more accurate in the former case than in the latter. There is a suggestion of uniformity in the value of the errors, and one cannot help thinking that

the everyday, haphazard activity, demanding as it does a certain degree of accuracy in the execution of movements, determines for each individual his range of error, and that time is the main element of variation.

It will be noted that though the time of the females is longer than that of the males, there is a compensatory increase in accuracy. The relation between time and accuracy has not been determined, so it is not possible to make a statement of the value of accuracy in terms of time, but undoubtedly the individual, who is fairly accurate and very quick, is more accurate when he takes more time, yet it is also true that he is sometimes much more accurate than at others without being necessarily either quicker or slower. These individual variations have still to be studied. The main point to be emphasized now is, that between two persons it is practically possible to bring one element of the test, either time or accuracy, to equality, so that the difference may be expressed numerically in terms of the other. Just what value this series of tests has can not be stated positively, but we believe it has distinct reference to motor ability, and that this will be shown by an increased number of observations upon individuals whose powers are definitely known by comparison with others in the various games.

I wish to acknowledge special indebtedness to Prof. Joseph Jastrow, Mr. G. W. Morehouse, Dr. F. B. Jewett and Mr. A. W. Jeardeau for assistance in getting observations.

DISCUSSION.

MIND AND BODY.

The question of the relation of mind and body is one from which the practiced reader shrinks as from a foreseen and profitless logomachy. It is, so to speak, a game of chess, in which the weary on-looker anticipates every familiar opening and every vain movement to the moment when the infinite baffler of our finite thought mockingly cries 'mate.' The deep-seated intellectual desire for unity and the impossibility of comprehending causal interaction between two disparate substances, drive us to the affirmation of a Spinozistic psycho-physical parallelism. Two courses then lie open to us. Common sense and the dread of paradox lead us to limit this parallelism, and to affirm that it obtains only in the nerve structures of the organic world. That is, that every mental event has its physical counterpart, but that the converse is not true. This view unflinchingly applied, together with the principle of continuous causation, makes the physical world the *prius* and the absolutely real, and reduces thought, feeling and consciousness to phenomenal illusions, *summis fluitantia rebus*. It is perhaps the dominant view as a psychological method at present. But it is not really tenable as an ontological principle by any serious thinker who knows his Berkeley, Kant and Schopenhauer.

The alternative is to make the parallelism absolute and assign a mental and subjective or 'inner' side, not merely to the nerve substances of man and the higher animals, but to every atom of cosmic dust. Mind thus becomes co-extensive with matter, and as the immediately known reduces its physical counterpart to an illusion, an inference, a presentation, an aspect or reflection of itself. But how are we to conceive this mind or minds? Are the barriers and limits imposed by matter as illusory, from the standpoint of the absolutely real, as matter itself? And are all that we count separate minds connected, related and fused in an infinite world-soul that manifests itself in countless finite aspects? Or is our indefectible sense of isolated individuality the mental counterpart of the lines of demarkation we find in the material world; and will eternal form continue to divide the innumerable minds the theory postulates? And if so,

what and where are the essential units of mind and reality? Is the unit the full consciousness of a mature man corresponding to the healthy action of the entire nervous system? Is it one of those split-off consciousnesses of which French pathology has so suspicious a monopoly? Is it a single thought, supposing us to be able to analyze out a single thought? Is it the dim sentience of an amoeba? Is it the postulated inner aspect of a molecule or rather, since molecules are compounds, of an atom? Evidently these questions throw us back into the metaphysics of the Leibnitzian monad, and we are led to speculate whether there may not be two kinds of spiritual unity,—one corresponding to the mental atom, the other to the various aggregations of such units under the control of a superior co-ordinating monad. By this plan, we might return to a world-soul that would yet leave us at least the illusion of a real finite existence. Speculations of this sort, alluring and inevitable as they are to all who dabble in metaphysics, do not touch the realities of our thought and experience very nearly. But however fruitless they may seem, they are at present inextricably involved with the methodology, the aims and the conflicting tendencies of modern psychology. No psychology in recent times is free from this sort of metaphysics, and the writers who protest against it loudest are the most deeply infected. Great interest attaches, therefore, to the review of the entire question just published by the veteran Wundt.[1] If we can not look for final solutions even from him, we may expect light on the darker places of his own voluminous works, suggestive criticisms of present psychological tendencies, and a clear defining of the somewhat obscure issues between him and the young psychologists of the school of Ziehen and Münsterberg.

It is probable that only a few very patient readers have been able to form a clear conception of what the controversy between these two schools, if we may call them so, is about. The new psychologists have all been directly or indirectly trained in the school of Wundt, and the master himself has from time to time modified the formal statement at least of his doctrines in concession to their criticisms. They profess a perfunctory allegiance, which he regards as hollow, to the results of Kantian criticism; and he disavows the traces of supernaturalism or mysticism which they discover in his theories of apperception, attention, and will. Both accept as fundamental the psycho-physical parallelism, the conservation of energy, the indispensability of introspection, the utility of the experimental

[1] *Ueber psychische Causalität und das Princip des psychophysischen Parallelismus.* W. Wundt, *Philos. Stud.*, X., 1–124. 1894.

method. Wundt denies that the psycho-physical parallelism involves the existence of any physical counterpart of the successive creative syntheses by which our thought is qualitatively elaborated out of the elements that analysis detects, and he stigmatizes as materialistic the psychology that fails to recognize this limitation on the study of mind through matter. But Münsterberg, while affirming that the entire content of our consciousness is explicable by the association of sensational elements in obedience to physical laws, expressly excepts from the possibility of such explanation the quality of consciousness that attaches to this content. May it not be that by this inexplicable quality of consciousness Münsterberg is merely generalizing what Wundt means by insisting that every mental synthesis yields a quality that cannot be obtained from the sum of its elements, and which is, therefore, inexplicable by any analysis or synthesis stated in physical symbols? If this is the case, there is, after all, no very serious philosophical difference between the two disputants. Both accept the psycho-physical parallelism, both recognize our inability to bridge the gulf between the two series. But Münsterberg, content with a general recognition of these difficulties, would simplify his psychological analysis by practically ignoring them, as does Spencer, and treating the mere combinations of the elements expressed in physical symbols as an adequate explanation of all higher states. Wundt, on the other hand, wishes us to recognize the irreducible quality of mind at each stage of the synthetic process, by which we rise from the simpler to the more complex mental states.

For the method and the language of psychology, however, the difference is all important. Wundt's psychological descriptions and analyses are couched in a literary language that makes its appeal directly to our conscious experience. The words are chosen for their power to recall vividly to the reader's mind the experience of which he treats. To some extent, of course, all psychologists use language in this way. But the tendency of the 'new,' the 'physiological,' the 'materialistic' psychology is to employ in psychological descriptions and analyses, language which has a merely symbolic and algebraic value, expressions chosen not for their power to reinstate the experience described, but for their convenience for expressing the writer's view of its explanatory analysis. Which dialect will the psychologist of the future use? If he is as clever as Prof. James he will probably employ both languages,—the language of vivid literary description to aid the reader in realizing the states depicted, the language of symbol to lend plausibility and a halo of science to the analysis.

But what is the real service of the symbol? In mathematical physics we substitute abstract symbols for the sensible realities, because the symbol is adequate for our purposes. It enables us to solve problems and to predict results. In other words, it represents the true causal relations so far as they are accessible to human intelligence. Now, the employment of symbols and symbolical, physiological language by the 'young psychologists' is mainly due to their instinctive desire to transfer to psychology the conception of cause which mathematical physics has made the ideal of modern science. Speaking of the schematic diagrams by which he illustrates association, Prof. James says: "It is only as incorporated in the brain that such a scheme can represent anything causal," and he accordingly denies that similarity can be an ultimate law of association on the ground that similar ideas do not co-exist in the mind in the intervals of latency, but are mere dispositions of the brain. A like feeling about psychic causality underlies his suggestions that psychology is awaiting its Galileo or Lavoisier, whose advent may surprise us any day. Our entire conception of the method and prosecution of psychological research will depend on our acceptance or rejection of this assimilation of physical to psychical causality. Owing to the homogeneity of its symbols (which enables us to interpolate imaginary links at pleasure,) physical causation tends to be conceived as a continuous unbroken chain. It is not easy to conceive of psychic causation in this way. Highly complex states succeed each other in the mind with no apprehensible intermediate links, and consciousness as a whole is suspended in sleep and disease. The resort to the infinitesimal of the unconscious savors of Leibnitzian metaphysics. It is easy, then, to see why the young psychologists seek to base their psychologies on the physical conception of cause. But we have still to ask, what is the justification of this procedure, either from the point of view of ultimate metaphysics or of practical psychologizing? If they seriously maintain that all causal efficacy resides in brain states which have no psychic counterpart, then, despite their professed allegiance to the critical philosophy, they are making matter a 'Ding an sich.' If they accept consciousness *in toto*, as a reality for which no physical conditions can account, or if they admit anywhere a mental spontaneity, which can select among the ideas which the associative machinery introduces, they have abandoned the unflinching mechanical explanation of the universe, as completely as if they granted us a soul, possessing the faculty of retaining latent ideas and associating them by similarity. If it is impossible to carry the mechanical explanation through, how

can they define *a priori* the powers and potencies of the irreducible spiritual factor whose presence in the problem is so grudgingly conceded? It will perhaps be said that this is unprofitable metaphysics, and that in practice the 'soul' has shown itself a perfectly barren and useless psychological conception, while brain processes and schematic diagrams have been found to be fruitful working hypotheses. What then is the real outcome, either for knowledge of the mind or for scientific anatomy of the hypothetical brain schemes that adorn the pages of the new psychology? Such are some of the chief problems suggested by Wundt's study, of which, after these introductory reflections, I proceed to give a brief summary.

Viewed in the light of the psychological origin of the conception, a cause is a thing that produces an effect on another thing. When primitive thought has occasion to distinguish cause and condition, it regards the thing as the cause proper and its varying aspects and relations as the conditions of its operation. More exact and abstract thought comes to recognize that there is always some special relation, quality or change of the thing that determines its effect on another thing, and thus arises a tendency to fix the attention on this determining relation or aspect as the cause proper, and to regard things with their complexes of qualities and relations as the conditions. Now in fixing the meaning of a term like cause, we may endeavor to make our definition include its psychological origin and popular acceptation, or we may intentionally modify the conception so as to make it a more convenient instrument of thought. In dealing with the idea of cause we must follow the latter course. We must modify the original conception in accordance with the needs of modern science. To attain command over nature and the power of prediction, science must possess a practicably applicable criterion for distinguishing that condition which is for our purpose the operating cause. From this point of view things are too vague to serve as causes. A thing is a complex of generalities, a seat of countless qualities known or unknown, the center of an infinity of relations, along the line of any one of which its qualities may operate. Yet to disregard things altogether, and to consider qualities and relations only, is to fall back on Hume's conception of causality, and retain no law or order in the world other than subjective rule of habit. This difficulty is in part met in the exact sciences by the modern conception of causality based on the conservation of energy and the equivalence of the forces of nature. The cause equals the effect in units of force, and the chain of causation is a series of mathematical physical equations. What is the significance, and what are the

limitations of this principle? In the present state of science it is a mere postulate. The complication of the problem, if nothing else, prevents verification in the majority of cases. But verification, when possible, is so precise, the evidence accumulates so rapidly, and the satisfaction of the imaginative desire for unity is so complete, that we assume the law to be absolute for the physical world. Does this mean that the physical order constitutes a closed series, into which it is demonstrably impossible to interpolate an alien or spiritual link, such as an impulse from the soul to the brain, however slight? On this point there is an apparent uncertainty in Wundt's utterances. The energy of position to be developed by a stone hurled into the air would remain the same, he says, were the stone arrested by a miracle and held in suspense for a given time. Now, only a small portion of the equations by which the mechanical explanation of the universe is stated are dynamic, we are in every case compelled to rest finally on static equations. And the validity of such equations, while not inconsistent with the conception of the physical world as a closed series, does not preclude the intrusion of an alien form of causality, provided the intruder is not supposed to create, but only to direct or release energy. But surely the only meaning of this is that gravitation, chemical affinity and electricity are still mysteries which we are unable to explain in dynamically continuous terminology. But the modern 'flowing philosophers' will claim that all reality is ultimately expressible in dynamical terms, and that the statical equations are mere temporary expressions of our ignorance. In which case they can only be met by pointing out that on this assumption every problem will be infinitely complicated and commit us to an infinite regress, making the mechanical explanation of the world forever impossible. Wundt virtually admits this. There are two conceivable types of miracles, he says: those that create new energy and those that merely release latent energy. The first are excluded by the law of the conservation of energy. The second become impossible if we postulate a continuous series of dynamic equations between any two statical equations. In any case, the burden of proof rests with the affirmer. Psychic phenomena yield no warrant for assuming miracles of the second kind, and the complexity and purposiveness, which we see in actions, known to be purely reflex and unconscious, are against it. Complete psychophysical parallelism, then, as an empirically given fact and a postulate of method, must be the doctrine of modern psychology. The metaphysical meaning of that parallelism and its application beyond consciousness, belong to metaphysics and epistemology. This

parallelism postulates co-existence in time between the associated members of the physical and mental series. But there are two psychic realities of which it renders no account. These are the combinations of psychic elements with each other and the *Werthunterschiede*. There is nothing in the world of physical forces and processes that corresponds with these.

Here again the young radicals will detect an irrational element in Wundt's philosophy, and it is necessary to define his meaning. Even assuming an established parallelism between the elements, physical combinations cannot explain psychic combinations because the former are quantitative while the latter are qualitative, and the product possesses qualities not found in the elements. This truth, the principle of creative synthesis, as he afterwards calls it, is a sufficient bar to all 'materialistic' psychology. But in the statement we are considering Wundt seems to have affirmed or denied more than is necessary. He denies not only that the physical parallelism can account for qualities, but that there is any physical counterpart. A feeling of inmost union between two tone or color sensations implies, he says, no physical bond beyond contemporaneity. This seems a wanton limitation of the principle of parallelism. The assumption that there is some sort of a physical basis for the qualitative likeness of feelings does not, as Wundt holds, lead to the *reductio ad absurdum* of a Cartesian pineal gland. There is ample room for imaginative conjecture in our ignorance of the structure and functions of the brain. The likeness may be conceived as represented on the physical side, not merely by physical contiguity but through connecting fibres or parallelisms of modes of motion. Such conjectures (which fill so large a place in the psychology of Herbert Spencer) have no anatomical value, but to deny *a priori* their possibility is to fling down the gauntlet against one of the most cherished scientific convictions of the day. Much of the same may be said of the exclusion of *Werthunterschiede* from the parallelism. Nobody claims that their quality is explained by any physical process. But neither is the quality of a taste or color so explicable. The analysis of *Werthunterschiede* into associated elements of pleasurable and painful feeling fills too large a space in contemporary ethics to be thus dismissed with a contemptuous *fin de non recevoir*. The psychic elements of pleasure and pain yielded by this analysis find their parallel in the furtherance or hinderance of the life of the organism. This is not, as Wundt claims, a mere transference of psychic *Werthunterschiede* to the physical side. Furtherance and hinderance,

as Leslie Stephen has shown at length, are defined in this connection by the Darwinian doctrine of survival.

These discussions are followed by an amusing and vivacious critique of the 'materialistic' psychology of Münsterberg, Ziehen and others. By materialistic psychology Wundt understands the psychology that ignores the equal validity of the psychic side of the parallelism, and deduces psychic events from physical. His weightiest criticism, however, is directed not so much against the false metaphysics of the school as against their doctrinaire simplification of the facts of the mental life, their persistent ignoring of the truth that for us the psychic side is in every case the most accessible. The essential vice of their method is that they do not patiently analyze the entire mental life as they find it, but conduct their analysis only with the view of winning hypothetical elements, (Bacon's *advolatio ad maxime generalia*), which are first correlated with elementary physical processes, and then combined in conjectural syntheses to account for everything. All the more complex facts of the mental life are then reconstructed *a priori* by physiological hypotheses about memory and sensory cells, connecting fibres and muscular sensations, without regard to the creative syntheses involved in all mental combinations. Where the quality of the products cannot be ignored it is attributed to the elements, and all possibility of further psychological analysis is precluded. Thus a special spatial and temporal quality is assigned to all sensations *per se*, and our complete intuitions of space and time are explained as summations of these. So Höffding, to account for recognition, assumes a 'quality of familiarity' resting on physiological habit, and all shades of feeling are explained as degrees of painful or pleasurable muscular and vaso-motor reflexes. In all these cases the true psychologist demands in place of this mechanical schematism an analysis of the complex state in mental terms, with distinct recognition and description at each stage of the analysis of the new psychic quality resulting from the combination. Even if we grant a certain symbolic truth to Ziehen's explanation of association by sensory and memory cells and associative fibres, in what way does so obvious a simplification forward our knowledge of the complicated interaction of image and symbol in our higher mental life? Does not the ready-made formula, here as elsewhere, check the patient analysis of detail, which is most fruitful in real knowledge? Wundt does not pause for questions of this kind, but makes merry with the whole theory, gravely suggesting that instead of saying the sensory cell deposits a memory in the memory cell, we should treat the memory

cell as an organism which feeds on the sensory cell, an hypothesis which he elaborates with somewhat ponderous Teutonic wit. Finally he points out that this hypothetical anatomy is of even less value to the physiologist than to the psychologist.

Much of the content of Wundt's final and longest chapter on psychic causality was anticipated in my introduction to this discussion. Specific psychic causality is found in all real processes of the mental life. For the intensive *quale*, the sensation, has real existence for consciousness only in space and time, and space and time, as we have seen, are products of creative psychic syntheses. Wundt's insistence on this point might seem an injustice to Ziehen, who explicitly recognizes that the projection of our sensations into space is "one of those psychological facts that are as yet incomprehensible in the light of physiological psychology, and that perhaps will always remain so." The fact is that in this controversy each party has borrowed all that is available of the dialectical equipment of the adversary, and that while they try to magnify their differences, their real difference is mainly one of taste in the statement of transcendental or irreducible problems. Wundt attaches value to the psychological analysis of our spatial perceptions. Ziehen does not; but it cannot be said that Ziehen attempts to explain space by physiological processes. Similarly of the controversy about memory, reproduction and association. The so-called laws of association are barren formulæ, he tells us. The true explanation of any concrete association is to be sought in the entire content of the consciousness as determined by the totality of its history and original endowment. This of course is an endless series. The physiological psychologists would simplify the problem by substituting the totality of the brain or nervous system, thus shutting us off from all real study of the facts. For psychological analysis can to some extent ascertain the facts in the past history of a given mind that determine a given associative reaction, but psycho-physics is limited to the barren generalization that the total consciousness is a reflection of the total state of the brain. The psychological analysis, however, will always remain on the border line between literature and science, and will not appeal to those who lack Wundt's literary skill or whose minds are dominated by the ideal of causation that prevails in the physical sciences.

In conclusion the author asks if it is possible to formulate definite laws of psychic causality. In the sense in which we speak of Kepler's or Galileo's laws, no. For the most characteristic psychic states are determinations of quality and worth, which do not admit

of quantitative formulation. By way of summing up the whole discussion, however, Wundt sets up two or three principles as specific notes or marks of psychic causality. First, the principle of pure actuality of process. This is in substance an adaptation to modern conceptions of Aristotle's doctrine that thought is pure energy and does not exist as a potentiality. Psychic causes in all cases must be real psychic events, for which it is not possible to substitute 'things' and their potencies, whether in the shape of a substantial soul, faculties, ready-made ideas or physiological processes. The temporary suspension of our consciousness is no objection to this view, Wundt thinks, provided a connection can be made out across the gap. How he would deal with double and split-off conscious personalities he does not indicate. This psychic causality is directly and intuitively perceived by self-observation, and thereby differs from physical causality, which is purely hypothetical and conceptual. It is here that the fundamental opposition between Wundt and the opposite school, who hold ultimately of Hume, appears. They would deny that we perceive in the sequence of our thoughts a constraining causal force any more than in the communication of force from one billiard ball to another.

With the principle of creative synthesis we are already familiar as the main idea of the whole book. The elements used by mechanical philosophers and psychologists in their constructions lack the qualities and the values of the real world of our experience. These qualities cannot be got out of the elements unless we grant the creative activity of the mind at each stage of the process. This principle, asserted against the Epicureans by the ancients, is a conclusive refutation of all attempts, from Lucretius to Herbert Spencer, to evolve the heterogeneous out of the homogeneous. Wundt is then sound in his main contention, as he is in affirming that the task of psychology is to trace the operation of this creative synthesis, rather than to elaborate unverifiable conjectures with regard to its physical parallels. What I cannot comprehend is his denial of the physical parallelism. Suppose, for argument's sake, that our analysis has reached elementary units. If these units are both psychic and physical, the psychic element has its physical parallel, which yet in no way accounts for its *quale*. Why, then, in the same sense, may not combinations of the one set of elements correspond in all cases to combinations of the other, though not explaining the specific quality that attaches to the compound? Will he say that it is because the only conceivable combination of the physical elements is spatial and temporal, and space and time themselves

exist only as products of the psychic syntheses, and imply a pre-existing consciousness capable of forming such syntheses? But is not this falling back into that idealistic denial of 'objects' from which we were told psychology (like the other sciences) must provisionally make abstraction? And does it not require for its explanation a complete statement of the idealistic philosophy of Wundt? And as I said in the beginning, can we be sure that it means anything very different from Münsterberg's admission that the fact of consciousness, as a whole, has no counterpart in the physical world? Thus while we all agree in deprecating the contamination of psychology with metaphysics, psychological literature is largely occupied with controversy over metaphysical conceptions introduced by the back door.

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ATTENTION AS INTENSIFYING SENSATION.

Dr. Münsterberg begins a paper on this subject (PSYCHOLOGICAL REVIEW, vol. 1, p. 39) by stating that the popular and generally received view is that attention does intensify our sensations. Common introspection certainly avers that attention as sensing effort generally, and within certain limits, is rewarded by increase of sensation. If I wish to hear better I listen harder; that is, I raise the sensation to stronger intensity by attending. I look for a dim star, I find it, and, increasing my cognitive effort, it appears brighter up to a certain maximum dependent on my state of health, training, etc. The keenest, most effortful glance gets the strongest sensation of a given light stimulus.

Fechner's observation that gray paper does not appear lighter the harder we look at it, does not destroy the fact that the more intensely we sense gray, the stronger is our sensation of it. And it must also be said that a dark object may with greater attention be discerned as gray, and dark gray as light gray; that is, where attention means not closeness of scrutiny, which often tends to close the eyes, but a wide open-eyed attempt to get full impression.

Further, the general theory of evolution leads us to suppose that only by attention has sensing and perceiving arisen and been developed in the struggle of life. It is by trying hard that the animal sees, and the harder it tries the more intensely it sees. Originally, then, a sensation becomes intense by attention as intensifying act, the sensing act is achieved and developed to various intensities only by and as cognitive effort. How is it, then, that an intense sound

gives us involuntarily, without the least effort, an intense sensation? This, we answer, is due to the efforts of ancestors for ages who at first were unable to hear the loudest sounds, but gradually achieved the hearing them, and hearing them intensely, and this tendency, perfected and integrated as useful in the struggle of existence, has been transmitted to us, in whom it acts automatically.

We believe, then, that if struggle or *nisus* is the fundamental power in the evolution of consciousness, then consciousness intensities of all kinds must be traced to attention; and so in the evolution of sense, attention is practically synonymous with sensation intensity. This does not deny that in certain cases of thoroughly integrated and automatic sensation cognitive volition when applied may be a hindrance, may reduce intensity and effectiveness of the sensation which always appear merely as a *given*. But this is of minor moment in a general discussion of attention. That we hear, and hear intensely, without listening, is because our innumerable ancestors listened, and listened hard. And so if future generations are to have certain forms and intensities of sensation *come* to them, it will be by our attention.

It may be evident, but it deserves emphasis, that we do not mean by sensation-attention, attending *to* a sensation. It is sufficient to know that where we now have intense sensations without intense attention, and without attention at all, this is not original and natural method, and that even now in general, when we wish to intensify our sensations, we exert cognitive effort, and with success. Expressed physiologically, it is the doctrine that function determines organ; that we see, not because we have eyes, but we have eyes because we see. The sensing effort has developed the eye, and by visual effort we now open the eyes wide and accommodate them, etc., thus securing intensity of sensation. Attending to a sensation is weakening to the sensation attended to. Thus, when absorbed in listening to music, some one asks me, 'Do you hear that false note?' the attention to the sensation as such weakens or destroys the sensation. Sensation-attention is not for us a consciousness outside of and directed to sensation, but sensing activity itself as cognitive effort. Nor is attention, as Mr. Shand implies, (*Mind*, Oct., 1894) a 'letting alone,' an isolating to see if a psychosis will strengthen, or will weaken and disappear. This hereditary spontaneous force of a cognition is the integrated result of past attentions, but is itself very different from attention as cognitive effort.

Believing, then, that sensation intensities are bound up with attention intensities as a general fact of mind, we were interested to

see how Dr. Münsterberg's experiments would bear on this law, to which he alludes in his opening remarks as the scope of his inquiry. However, we discover that it is only a certain kind of attention, expectant, and a certain kind of this, too much expectancy, that is really treated, with the result that sensations of light, sound, etc., are rendered less intense when we set our attention at too high a notch. The familiar experience of lifting falsely estimated weights is appealed to in a general way, but let us particularize. I see a two-pound wooden ball, which I take to be a ten-pound iron ball, and making muscular pre-adjustment, according to my misjudgment, it lifts 'as light as a feather;' I do not get the impression of a two-pound ball lifted with more just preparation or in a mere casual way. Thus sensations of weight may in intensity be inversely relative to the effort put forth. So if I am bid to look for a bright light or listen for a loud sound, and only slight stimuli actually occur, the sensations will be actually slighter in intensity than they would otherwise have been; the light does not seem so bright nor the sound so loud as when no pre-adjustment has been made. Now this result is not really 'unexpected,' but is quite the 'popular view.' We all know the answer which is commonly returned to those who realize certain sensations rather feebly, 'you set your hopes too high.'

But pre-adjustment may be too little as well as too much—a fact which Dr. Münsterberg does not notice—and the consequence is an undue increasing of intensity of sensation. When coming down stairs, and inadvertently taking two steps at a time, you have a peculiar abnormal increase of intensity of sensation, seeming to drop a very long distance, altogether disproportional to the actual distance, and this result is plainly owing to the wrong degree of pre-adjustment. Similarly for sensations of light, sound, etc., it is a common experience that under-adjustment means over-intensifying sensation. It is only when pre-conception and pre-adjustment are in a certain exact relation to stimuli that sensation occurs without abnormal heightening or lowering. It is well known that reaction-time is lessened by correct expectant attention.

The conditions and methods of the experiments call for some criticism. The agents were directed to have their attention 'fully' occupied with adding numbers. Now, adding is a process which is with most educated persons more or less automatic unless at top speed. But what is 'full' attention? Is it a scientifically determinable state, and one which can be induced as readily as securing air full of moisture at what we term saturation point? How can the experimenter be sure of attention at a certain degree? The inexact-

ness of experimental psychics as compared with physics is certainly great. The intensity of cognitive effort is neither easily discernible or measurable. However, we may say this, that attention at its strongest, is complete absorption of psychic capacity, and an agent in this state as regards adding effort would be entirely insensible to any stimuli. Further, full attention can only be reached by the full interest. When life for a wrecked sailor hangs on his seeing a certain beacon, then the intense interest is secured which assures intensest attention. But having, if possible, secured the highest attentions of several individuals, these attentions cannot be lumped together as identical in value. The tensile strength of iron bars of a given quality may be determined as equal, and the results used as a general value; but attention, as all mentality, has an individual equation. Again, it may be that the method of measuring intensity of sensation employed is the only feasible one, namely, judging intensity by the estimation the subject puts upon intensity of stimulus, still its inexactness is obvious. If a man says after lifting two rocks, one is twice as heavy as the other, are we thereby certain that one sensation was twice the intensity of the other, or may not the man judge also from other methods? At least the method ought not to be assumed without criticism and validation.

Further, Dr. Münsterberg finally explains attention as reducing intensity of sensation by feeling of strain. A tenseness of attention introduces a feeling of strain, which decreases the sensation felt. Sense of strain is undoubtedly divisive of consciousness, if with Dr. Münsterberg we interpret it as feeling of intensity. However, we do not gain anything by considering intensity of feeling as feeling of intensity. I have an intenser light sensation from the sun than from a candle, but if I say the intenser sensation is such by virtue of the greater feelings of tension, this means no more than that intensity of the sensation depends upon a sensation of an intense sensation, which latter intensity has to be explained, and so on. However, as I have pointed out, (*Mind*, XIV., p. 538) it is desirable to understand intensity not as a consciousness, but, like duration, as a quality. Every consciousness, including consciousness of intensity, has its intensity, which may or may not be felt or attended to. Strictly speaking, the intensity of my sensation—a feeling intensely—is never feeling of intensity, consciousness of intensity.

Still further, the 'chosen graduation of the stimuli' must be justified in the light of Weber's law, and the time intervals must also have their justification. The relation of change in time in attention and inattention and unattention to the problem of sensation inten-

sity must also be investigated if any satisfactory result is to be obtained. To get exact results in experimental psychology in general is then, I am persuaded, an enormously difficult task. The complexity of adult human consciousness is so great that it seems well nigh impossible to isolate the factors we are studying, and to secure identical reactions in a sufficiently large number of cases to prove a psychic law. Physical conditions are far more under our control than psychical, and are far easier to observe, and hence physical science has arrived at a *consensus* which is notably lacking in psychical. Essential preliminaries must first be settled before experimental psychology can really be fruitful, and the relation of attention to intensity of sensation requires far closer definition of subject and method than has yet been given it, if results of large scientific value are to be obtained.

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PLEASURE-PAIN AND EMOTION.

Serious and courteous criticism from the pen of a thinker, skilled in the subject of discussion, is certainly in all cases to be welcomed by an author, and I feel much gratification in reading Dr. Santayana's remarks upon my lately-published book[1] in the July number of this REVIEW.

There are one or two points raised in the review which I think it worth while to discuss.

In the first place, in the interest of psychological advance I must deprecate the implication of the opening paragraphs; viz.: that the writer of what aims to be a scientific discussion of pyschologic doctrine is no great sinner if he consider the claims of literary æsthetics in his exposition, where there is the slightest chance that the clearness and definiteness of his meaning may thereby suffer.

I regret much more than my critic can do that the book is so unattractive in its literary quality, but on the whole I do not feel confident that I could have made it more pleasing had I not deemed it of the utmost importance to aim at accuracy and to waive verbal preferences in favor of precision.

I am free to confess that the reading over and over again of my proofs has produced within me a deep-seated digust with many phrasess in the book, notably with the compound word *pleasure-pain*; but what authorized substitute could I have used in this case save the word

[1] Pain, Pleasure and Æsthetics.

'feeling'?—a word which is truly much more euphonious than the one employed, but entirely devoid of accuracy. I do not wish to excuse the evil complained of by my critic, a cleverer writer might have overcome it; but I think that it would have been all wrong to have chosen in any case literary worth as against definiteness, in such a work.

I raise this point principally because I feel that psychologists to-day are too often careless in this regard. They are too apt to discard in disgust an awkward but accurate term or phrase and to use in its place something of better æsthetic quality but decidedly inferior in definiteness. Or they go even further and add emphasis to unimportant particulars by the attractive nature of some form of speech or of some chance illustration. The extraordinary misconception of Prof. James' emotional theory by other psychologists, to which he draws attention in the September number of this REVIEW, may in my opinion be partly accounted for in this way. I have in mind a case in which I myself entirely lost the drift of an interesting argument presented in a paper read before the last meeting of the Psychological Association, because my mind refused to be dragged away from the æsthetic contemplation of a happily-used and beautiful quotation to the hard thinking required in following the course of the argument.

In the field which I touch the preference of euphonious but inaccurate terms and phrases, where 'barbarous' but accurate ones could be found, has been especially unfortunate in result.

I am very sure, for instance, that much of the voluminous literature of the subject of which I have treated would have remained unpublished had the authors avoided the use of '*Gefühl*' in German and '*Feeling*' in English. Had they used 'pleasure-pain' (or some better equivalent) when and only when they meant it and nothing else, many of their most effective periods would have become evidently illogical or irrelevant. My critic shall furnish me with an example of the danger. It is much pleasanter to speak of the æsthetic as determined by pleasures of *memory*, than by pleasures of *revival*; and to avoid repetition I did give way once, I believe, and use that term in one of the statedly popular summaries. But the pleasures of *memory* are not all that I refer to. A memory is a special kind of revival. *Revival* therefore is a much broader term than *memory*. What I refer to are *revival* pleasures, and the use of the word *memory* in this connection at once limits the thought of the reader to definite objects: With my critic, the notion that the two

terms are interchangeable has led him at times to misconceive to some extent the thesis presented for examination.

What has worried me indeed has been not so much the failures of style, to which Dr. Santayana calls attention, as the consciousness that I may possibly have been guilty of the very faults of inaccuracy that I deplore in others.

But to turn to the criticism itself, I have no desire to combat objections raised except where they seem to involve misapprehension, and I am glad to say that I think all of Dr. Santayana's oppositions, as expressed, will disappear upon a clearer apprehension of my meaning.

I hold that there is no "clear distinction between the sense of pleasure and the sense of beauty" *in impression:* and these last two words, that I add, are of the very essence of my thesis. The distinction which is noticed is one made in judgment upon revivals. With these two words added to Dr. Santayana's expression I have no hesitation in leaving the cases he brings forward to introspective tests. I am sure that for myself when "I have no definite object before the mind, but am lost in a torpid reverie" which is pleasant, the state of *impression* is indistinguishable from many of the impressions that are *called* distinctly æsthetic; *e. g.*, the impressions coming to me as I listen to some parts of Wagner's 'Tristan and Isolde'; and I have no hesitancy in holding that if an object *afterwards to be judged beautiful* were to appear in connection with this reverie, the pleasures of æsthetic *impression* connected with this object would completely fuse with those of the 'torpid reverie.' The point of difference lies just in the distinction between the æsthetic judgment and the æsthetic impression, the former of which always relates to objects or objective states. In this particular case the revival of the state of 'torpid reverie' is necessarily associated with the torpid object, and for most people such torpid objects or their mental states are, in revival, so very insipid that they cannot be noticed to be pleasurable and are therefore *judged* to be unæsthetic. Dr. Santayana says "but this pleasure" (of torpid reverie) "would not be æsthetic, because I could not perceive any beauty, seeing that no object is present to me in which that beauty may reside." By the words 'would not be æsthetic' he certainly means "would not be called or thought of in retrospect as æsthetic"; he is speaking of what I consider to be a judgment as to the nature of revivals. His words, however, would lead one to think that he considers this phrase to relate to the direct nature of the impression.

The other case mentioned by my critic is also, I think, distinctly in my favor. It is perfectly true that "few pleasures are so vivid in revival as those of satisfied vanity, affection, revenge and other personal passions," and so far as I have indulged myself in these intoxicants I feel sure that I have been æsthetically *impressed* at the time. I am unable to draw any distinction between these pleasurable *impressions*, so long as they remain mere *impressions* and those other *impressions* produced by what is acknowledged to be beautiful. The distinction comes in the revivals upon which we act in judgment, when the despicableness of the self-complacency brings a balance of pain to a man who is properly constituted. My critic's examples, indeed, are not here very forcible, for, in the revivals of the 'personal passions' mentioned, I am usually distinctly judging of myself as worthy in some respect and therefore as an æsthetic object.

There is another direction in which I wish to make my position clearer. I am one of those who think that too much emphasis is given to-day in some quarters to the physiological basis of psychology. I am heartily in sympathy with any investigations that can throw light upon psychology, and I think the patience and persistency of our experimenters in psychophysics is most noble, and, except so far as it is misapplied, it certainly should be most heartily encouraged and applauded. On the other hand, I feel with many of the advanced neurologists that we can only claim to be *beginning* to understand the nature of those neural changes which form the basis of psychic life. I do not feel sure that our present notions of the relation between mind and body may not seem very crude in a few centuries from now, just as those held by the Greek philosophers do to us to-day.

Just here it will be convenient and appropriate to call attention to a point which relates to this subject-matter, and which supports the view just expressed. Two years ago Dr. H. Nichols published in the *Philosophical Review* a defence of a theory that pains are a species of sensation. I argued in a reply, which appears again in my book, that this view is opposed to psychological evidence, and that the facts, mainly physiological and histological, upon which the theory depends for its support are, with possibly one exception, entirely compatible with other deductions than those made by those upholding the sensational theory. This one exception was the claim made that Goldscheider had discovered definite nerve-terminals for pain in the skin. I objected that in this field the statement of one observer of a limited number of subjects should be received with caution, and I further noted that Goldscheider had *implicitly* denied the

position involved in his first statement as it was interpreted, and upon which interpretation Dr. Nichols founded his argument. Dr. L. Witmer has lately reiterated Dr. Nichols' theory in the *Journal of Nervous and Mental Diseases* for April, 1894, and has sharply called me to task for being unwilling to accept as final Goldscheider's supposed dictum. Dr. Collins, in his review of my book in the same journal, has made the same criticism of my position in this respect. But now there comes to hand a new book by Goldscheider.—'*Ueber den Schmerz*,' Berlin, 1894,—which serves I think to teach a lesson to all psychologists, and especially to those who may have taken interest in this discussion; for in this book Goldscheider distinctly denies the view which has been thus attributed to him, and seems to think he cannot properly have been held to be a defender of a position so evidently untenable; although I think his *words* in his early publications certainly spoke clearly as they have been understood. Goldscheider now holds (p. 7) that *Schmerz* is "eine besondere Qualität der Empfindung, nicht eine allen verschiedenen Qualitäten gemeinschaftliche Modifikation der Empfindung." Further (p. 13), "dass die Schmerzempfindung den Drucksinn and Gemein-gefühlsnerven eigen ist, allen übrigen Sinnesnerven aber fehlt." I do not appreciate upon what sufficient grounds he bases his belief in the existence of these Gemeingefühlsnerven (see also p. 33) and of the Gemeingefühlserregungen spoken of elsewhere (see p. 8). He tells us further (p. 18), "Hiernach läge es in der That nahe, jeden Schmerz als ein Summations-Phänomen anzusehen, allein dies gilt nicht ausnahmslos": and he postulates a 'Summations-Organ' (see p. 34) located in the spinal cord (see p. 19) to account for the effects of pain. It does not seem to me that we should receive without caution the statements of an investigator who makes such free use of unverified hypotheses.

Goldscheider in this new treatise, if he does nothing else, shows conclusively that our knowledge of the nature of the neural changes which are the coincidents of pain consciousness is of the most indefinite character, open to dispute in every direction, and that no physiological or histological theory relating thereto can to-day be held to be proved. Moreover, so far as I can see, there is little reason to lead us to hope that we shall be able to reach any settled position in this respect in the near future.

This occurrence strengthens within me the conviction with which I wish more of our psychologists clearly showed their sympathy, that introspective psychology must move on in her development without waiting for the positive teaching of psycho-physics; she

must of course endeavor to check the results of introspection by what becomes known through psychological investigation ; but it would surely be a great loss to philosophy and to science in general if psychology hesitated in her course while awaiting clear light from this source.

This being my view, it was with regret that I found it impossible to discuss adequately in my book that which has been done in the past in reference to the physiological basis of pleasure and pain, and to suggest the direction in which the facts before us vaguely point, without giving relatively much more space to the subject than its importance warrants.

I am disappointed, moreover, to find notices of the physiological theory so prominent in the reviews of the book, and especially to find Dr. Santayana in this review taking for granted that the basis of my æsthetic principles is to be found only in this necessarily vague physiological theory. In fact if the reader will take the trouble to examine the matter he will find that the basis of these æsthetic distinctions and principles is really determined by introspective evidence and not derived from physiological hypotheses ; and that the physiological correspondence, as it would appear under my theory, is generally stated in the chapters dealing with these principles, in small print in brackets. Thus it appears indeed that the psychological æsthetic principles give us a very strong corroboration of the physiological theory, but I do not feel that it is evident that these æsthetic principles necessarily fall if the physical theory crumble into dust, as is implied in this review, and as has been asserted by Dr. Jos. Collins in the review in the *Journal of Nervous and Mental Diseases* above referred to.

It is because I feel the secondary importance of this physiological view that I am also greatly disappointed to find my critic holding that my main *psychological* thesis turns upon a mere matter of words. I had hoped to show that this psychological thesis has strength; and that, so far as our knowledge of the physiological aspect of the subject goes, the psychological view is not incompatible with that knowledge ; for I seem to see that if the psychological view be true, and if it be carried out to its consequences, it may lead to results in other directions than those especially studied that may prove to be interesting at least ; and I had hoped that the results as brought forward in relation to the emotions, the art impulse, and the principles of æsthetics, might appear to be not wholly valueless, altogether apart from any physiological theory whatever. As this psychological theory is in my view thus important, I trust that it will not appear

out of place if I try to convince my critic that it is a great deal more than a mere verbal contention that I make.

In the first place I may again call attention to the dangers attendant upon the æsthetic treatment of what should be strictly accurate science. Dr. Santayana's statement of my proposed definition of pleasure and pain is probably pleasanter to his ear than my own. He states that I hold them to be "qualities either of which may and one of which must belong to *every perception of the mind.*" But this is not the doctrine I have expressed, unless my phrase 'each element of consciousness' is made equivalent to his 'every perception of the mind.' I do not think the two expressions are at all synonymous, and I believe that one must avoid the statement as made by him if one is to grasp correctly the thesis and its implications.

But passing over this inaccuracy, let us turn to his argument itself. It is perfectly true that we may equally well say "that color is a quality of extension, or that they are two simultaneous perceptions, or that they are both qualities of a present substance." But is it no gain to take note of the fact that at times when there is no color perception at all there still may be a consciousness of extension; that other sensations and mental modes than color sensations have this consciousness of extension connected with them? Is no importance to be attached to the thesis that *extensity* may be a quality of very wide application? Can it justly be held that the contention of the present day in relation to space, which turns upon this thesis, and in which the greatest psychological thinkers of our time are involved,—that this contention 'turns upon a matter of words,' '*becomes real and not verbal only in the field of physiology*'?

Or to take another instance more to my liking. The *intensity* of a color may be treated as Dr. Santayana treats its extensity. It might be said that the facts may be "described equally well by saying that color is a quality of" intensity, "or that they are two simultaneous perceptions, or that they are both qualities of a present substance." But is it no gain to psychology that more or less of intensity is acknowledged to be a quality attached to all elements of consciousness? Would it be of no value to make contention for this doctrine were psychology in so crude a state that some masters held 'intensity' to be a species of sensation; others that it is a kind of emotion; others that it is the fundamental basis of all psychic life; others of great weight that a special kind of mind, apart from our cognitive mind, must be postulated to enable us to grasp *intensity* —*i. e.*, that *intensity* is a mental mode *sui generis*? Could such con-

tention, if it were necessary, be held to turn upon a mere matter of words, and to become a real question 'and not verbal only in the field of physiology'? It is such a contention as this that I make for pleasure-pain.

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NEW YORK.

A COMMENT.

In Wundt's article, *Zur Lehre von den Gemüthsbewegungen* (Phil. Stud., vi, p. 364), occurs the following passage:

Die Apperception selbst ist nichts, was den Effecten, die sie am Vorstellungsinhalte erzeugt, und den Begleiterscheinungen, die sie im Gebiet des Gefühls hat, als etwas besonderes, realiter zu trennendes gegenüberstände. Vielmehr besteht sie selbst nur aus diesen Begleiterscheinungen und Wirkungen.

Professor James exclaims, àpropos of the last sentence: "A thing that 'consists' of its concomitants!" (This REVIEW, I, p. 516.) The exclamation is hardly fair criticism. We read, p. 390 of the same article:

Zu jenen Begleiterscheinungen reche ich in erster Linie gewisse zu Vorstellungen vereinigte Empfindungen, in zweiter Linie die Willensacte theils vorbereitenden theils mit ihnen unmittelbar verbundenen Gefühle. Die letzteren lassen sich jedoch nur auf Grund der einmal vollzogenen abstracten Unterscheidung zwischen Fühlen und Wollen Begleiterscheinungen des Willens nennen. In Zusammenhang mit der Entwickelung des Willens betrachtet, verwandeln sie sich selbst in Elemente der Willensthätigkeit, die sich aber deshalb, weil aus ihnen nicht immer ein actuelles Wollen hervorgeht, nun auch in solchen Fällen, wo dieses eintritt, demselben als begrifflich trennbare Bestandtheile gegenüberstellen lassen.

It is, of course, really not much fairer to quote two passages without context than it is to quote one. But the second of the above citations may serve to show that Professor James' scorn is not so undoubtedly merited as might at first sight appear.

E. B. TITCHENER.

PSYCHOLOGICAL LITERATURE.

HALLUCINATIONS AND TELEPATHY.

Ueber die Trugwahrnehmung (Hallucination und Illusion) mit besonderer Berücksichtigung der internationalen enquête über Wachhallucination bei Gesunden. EDMUND PARISH. Leipzig, Abel, 1894, [Schriften d. Ges. f. psych. Forschung, Heft 7–8; II. Sammlung] Pp. 246.

The erudition of Herr Parish's work is exemplary and admirable, and in its text and footnotes it is safe to say that one may find reference to everything, important and unimportant, that in recent years has been written on hallucinations from either the medical or the psychological point of view. The author's personal contributions to the subject are animated by the laudable desire to minimize mysteries and to explain the exceptional phenomena of which he treats by the laws of ordininary mental life. The important points in the book are, first, Herr Parish's general theory of the hallucinatory process, a theory which he applies to all possible cases; and second, his verdict of *non liquet* upon the telepathic theory of veridical hallucinations maintained by the English 'psychical researchers.'

His theory of the hallucinatory process is that it is always an incident of 'dissociated' conditions of consciousness. By a dissociated condition he means one in which ordinary channels of association are obstructed. Reviewing the conditions under which hallucination is apt to occur, he finds them predominantly to be of this sort. In sleep, in the borderland between sleeping and waking, in melancholy, in hysteria, epilepsy, the delirum of fever, of fasting, and of certain narcotic poisonings, in hypnotism and crystal gazing, the fact of obstructed associations is admitted by all. Even in mania and drunkenness, where association seems at first sight rampant enough, this is chiefly verbal association, and objective thought is enfeebled and slow. The way in which dissociation facilitates hallucination is according to Herr P., this[1]: A stimulus is always drafted off into the most pervious paths at the time being. In normal association

[1] Herr P. expressly bases his theory on that of the hallucinatory process given in James' Principles of Psychology, II. ii.ff.

these are the most habitual paths. But there are always many stimuli at work, and many 'cerebrostatical' conditions determining perviousness, so that the final process aroused by a stimulus is the result of an intricate array of factors. Whatever path is followed *to a pause*, gives there a vivid sensible content which, in normal cases, involves a veracious perception of the object from which the stimulus comes. But if at any moment a dissociative condition is realized, so that the usual paths are blocked, whilst at the same moment other accidental paths are in a state of exalted tension from inner causes, then into these latter the stimulus discharges its energy, making them explode with the maximum of force; so that the result is the perception of an object having no usual connection with the stimulus, and by the vividness of which the consciousness of the latter may be eclipsed. The reigning state of obstructed association moreover weakens the subject's critical reaction, and the false perception is not only experienced but believed. This theory is ably defended by our author, and has the merit of being very general, and of bringing hallucinations and illusions under a common law.

Do the sporadic waking hallucinations inquired into by the 'Census' of the International Congress of Psychologists easily fit under this law? Our author tries to make them do so. First he attacks the truth of the Census, in which 'borderland' cases are hardly more than half as numerous as the 'waking' cases. Considering this to be *a priori* impossible, he explains the actual statistics plausibly enough by the greater tendency of the borderland cases to be forgotten (it being already demonstrated that the majority of all hallucinations *are* forgotten). Next, taking the alleged waking cases, he shows by a number of examples that in them also dreaminess or some other dissociated consciousness may be supposed—The Subject was 'fixating' something, if no stronger reason can be alleged. I must say that Herr Parish seems to me here to drive his theory a little too hard. Many of the narratives so distinctly belong to normal consciousness, that the better tactics would be to discredit their veracity altogether; and this method also, Herr Parish applies vigorously to the particular class of hallucinations called veridical or coincidental (*e. g.*, with the death of the person perceived).

Prof. Royce's suggestion that the narratives are often due to 'pseudo-presentiment' (false belief, after the death has happened, that it had been symbolized by an apparition previously) is made liberal use of, in spite of its almost absolutely conjectural character. The much sounder objection follows that genuinely occurring hallu-

cinations are equipped afterwards, by the retrospective imagination of their percipients, with details that fit those of the event with which, when it happens, they are supposed to be connected. This especially applies to them where they are collective, the different percipients obeying each other's suggestion as to what they saw. Finally the false appearance of frequency of hallucinations of the coincidental class is explained by the far greater tendency of the non-coincidentals to become forgotten, the coincidentals resisting oblivion. Furthermore, Herr Parish contends that the 'frequency' of the coincidentals should in any discussion as to their being due to chance be set down as the ratio of their number to that of hallucinations of all varieties, and not to that of their own variety, which in the argument of the English committee is defined as that of 'apparitions of recognized living persons.' For all these reasons, Herr Parish concludes, the alleged frequency of the veridical class of hallucinations becomes so reduced as to form no argument against the genuine cases among them being due to chance. Moreover, he adds, we cannot lump the cases in one order of probability. Where for example the percipient is the anxious child of an aged parent ill with pneumonia, the chances are that if she have an hallucination at all, it will have that parent for its subject.

Herr Parish's criticisms are partly based on the provisional report of the English committee published at the International Congress of 1892. The committee have themselves considered such objections in their final report, which forms the subject of our next article. So I will immediately proceed to give some account of that. I will say meanwhile that this German critic's tone is uniformly respectful; that he himself prints the 59-yes cases of the Munich Census, of which 11 are more or less coincidental; and finally that his work is the most solid existing contribution to the subject up to the date of the report whose title follows below.

Apparitions and Thought Transference, an Examination of the Evidence for Telepathy. FRANK PODMORE. Contemporary Scientific Series. New York, Charles Scribner's Sons, 1894. 12°, pp. 401.

Mr. Podmore gives here a convenient summary of the work of the Society for Psychical Research, striving to make the theory of telepathy cover as much of the field as it can be stretched over. When one sees brought together, as here in the early chapters, the evidence for thought-transference drawn from the simple experiment in which one person is set to guessing numbers, drawings, etc., which another person is intently looking-at or thinking-of, one perceives

that it is far from contemptible in either quality or amount, and even if one is unwilling oneself to follow, one can find no very harsh names to apply to those who, like Mr. Podmore, take thought-transference as an approved *vera causa*, and try by its means to explain such phenomena as apparitions at the time of death, distinct in nature as they appear at first sight to be from the successful guessing of pictures in another's mind.

The book mentions successful experiments of the simple order with at least thirty subjects at short-range, and this leaves out many of the records published in the S. P. R. Proceedings. Of course these experiments are of diverse value, some of them being too brief or too faulty in method to base strict conclusions on, but they all contribute to the cumulative impression that chance and trickery can with difficulty be supposed to be the only things concerned. As an instance of a good series I take the observations of Mrs. Sidgwick on five hypnotized subjects who guessed numbers drawn by a third person from a bag containing 81 lotto-counters marked from 10 to 90, and handed to the hypnotizer to gaze at, all this of course out of sight of the subject. Out of 644 trials 131 were successful, that is, both digits were given correctly, though in 14 out of the 131 cases the order was reversed. 'Chance' should only have given 8 correct guesses. Again, with hypnotizer and subject in different rooms, there were 27 quite correct guesses, instead of the chance-number, 3. In the unsuccessful trials here, the first digit came right 85 out of the 252 times, instead of the chance number, 28. Mrs. Sidgwick went through another series with the same subjects, in which 'mental pictures' were the things to be guessed, some of them being quite complex scenes, in all 108 experiments, of which 33 were correct. Of these trials, 55 were made with the agent and percipient in different rooms, so that the successes in the same room were 31 out of 71. Practically, since collusion seems fairly excluded, the only recourse of the doubter here is to say that the series were too short and that farther experimentation would have reduced the success to the chance-number. And here is where the force of so many other successful series, longer or shorter, comes in. They make the reader feel as if the dice must be in some way loaded; and to the force that loads them Mr. Podmore and his colleagues have given a name, that, namely, of *telepathy*, in lieu of a theory about it.—It is clear that many series of guesses with more successes than the probability due to chance can yield, will not positively prove that chance may not have produced the result after all; and it is still clearer that such statistics are no guide as to what the

positive force may be. And here the other phenomena gone over by Mr. Podmore come in to give some feeble help. But they run into a mass of details ill adapted for synopsis, so with this brief notice I conclude.

Report on the Census of Hallucinations. H. SIDGWICK, A. JOHNSON, F. W. H. MYERS, F. PODMORE, E. M. SIDGWICK. Proceedings of the Society of Psychical Research. Part XXVI. Aug., 1894. Vol. X., pp. 25–422.

This extraordinarily thorough and accurate piece of work is understood to be the fruit mainly of Mrs. Sidgwick's labors; and the present reviewer, who has had a little experience of his own with the 'Census,' and knows something of its difficulties, may be allowed to pay his tribute of admiration to the energy and skill with which that lady and the other members of the committee have executed their burdensome task. They collected no fewer than 17,000 answers to the question: have you had, when awake, etc., an hallucination, etc. Of these answers 2,272 were 'yes,' and these Yes-cases were corresponded with or interviewed or in other ways subjected to as critical a scrutiny as circumstances allowed. The result is an unusually careful handling of the raw material offered, and a great accession of new facts. The census of hallucination was, as is well known, an idea of the late Edmund Gurney, who thought that the theory of chance-coincidence applied to 'apparitions' reported as occurring on the day of death of the person appearing might be tested by statistics. Gurney himself collected 5705 answers, and, applying statistical reasoning to them, thought it superabundantly proved that the 'veridical' cases amongst them were too frequent to be due to chance. The Sidgwick report, unlike that of Herr Parish, keeps the Gurney question well to the front, and its general discussion of the physiological and other conditions of the hallucinatory process is less erudite and elaborate than that of the German writer.

I will quote immediately the conclusions of the report as to apparitions at the time of death. "We have 30 death-coincidences in 1,300 cases [of visual hallucination of recognized living persons] or about 1 in 43. But chance would . . . . produce death-coincidences at the rate of 1 in 19,000 apparitions of recognized living persons, and 1 in 43 is equivalent to about 440 in 19,000, or 440 times the most probable number. Or, looking at the matter in a different way, we should expect that if death-coincidences only occur by chance, it will require 30 times 19,000, or 570,000 apparitions of living persons to produce 30 such coincidences. . . . . We con-

clude then that the number of death-coincidences in our collection, if our estimate of them is accepted as fair, is not due to chance. This will not be maintained by anyone with the most elementary acquaintance with the doctrine of chances. The opponent of a telepathic or other supernormal explanation must take one of three other lines of argument, . . . even one death-coincidence being more than we should be justified in expecting chance to produce in a collection ten times the size of ours" (p. 247–8).

Everything in this conclusion depends on the numerical premises being severally reached in legitimate ways.

In the first place, take the assumption that out of 19,000 apparitions of the sort considered, only 1 should be expected to occur on the day of death of the person seen. This is based on the mean death-rate of England. Since in England the mean annual death-rate at present is 19.15 per 1,000 of population, the mean daily death-rate must be 365 times less, or 1 in about 19,000. All daily operations concerning persons, if not directly contingent upon their death, would under these conditions be more likely to strike the living than the dying in the proportion of 19,000 to 1, and this no matter how frequent or infrequent absolutely such operations should prove to be. Apparitions are operations concerning persons; and whether such apparitions be as frequent as dreams, or whether they be very rare, whether a large fraction or a small fraction of the population be visited by them, we should expect (if they be due to mere chance) always to find this proportion observed, that only $\frac{1}{19000}$ of them should be of people who were dying on the day when their apparition took place. [This 'day' is measured in the report by the 12 hours preceding and the 12 hours following the death.] To the present writer this reasoning and computation seem valid.[1]

[1] In particular does the contention of Herr Parish (see the article on him, above, *ad finem*) seems inadmissible. He says that in estimating the probability that apparitions at the time of death are due to something more than chance we ought to measure their frequency by the ratio of their number to that of *the aggregate of all phantasms of whatsoever description*. He would even include illusions, since the process of illusion and hallucination are for him fundamentally the same. To base an argument on the ratio between the number of veridical death-apparitions and that of merely *all apparitions of recognized living persons*, he says, is a *petitio principii*. The point is a subtle one, and may well make one momentarily hesitate, but reflection leaves no permanent doubt. We have three orders of frequency in hallucinations to consider, that of hallucinations at large, that of hallucinations of persons, and that of hallucinations of dying persons. These may be caused by their respective objects, or may come at 'random,' their causes lying exclusively in the subjective cycle. The point is to see whether anything in the frequency itself can help us to decide which of these alternatives is the true one. Now with what frequency in outer things might these frequencies

Next, how are the numbers 1,300, for the whole number of visual apparitions of recognized living persons, and 30 for the coincidental ones among them, established? Neither of these numbers is that of the crude face of the census-returns, each being a number estimated by applying certain corrections to those returns, the corrections all being such as to weight the figures in favor of chance-coincidence as far as this can with any plausibility be done. The crude returns certainly include an unduly large percentage of coincidental apparitions, partly because a large number of non-coincidental ones are speedily forgotten and do not figure in the returns, and partly because, of the coincidental ones, some are likely to have been put in by careless collectors on account of that character, and not to have simply turned up in the census-taking by due process of chance. Now can any definite estimate be made of the amount of error that has crept into the census from these sources? The authors of the report find, by comparing the dates of the returns, that cases are the more frequent the more recent they are. This proves a forgetfulness increasing with antiquity. The obvious remedy would be, ascertaining what recent period could be taken as trustworthy, to find out how many hallucinations had visited the persons figuring in the census during that time, and then to treat

in hallucinations keep tally in the two cases, of outer causation and of no outer causation respectively? Obviously if persons do not cause hallucinations of themselves, the hallucinations of persons should be *no more* frequent among hallucinations than persons are frequent among all the things that may become objects of hallucinations; whilst on the contrary, if persons, and persons alone, do cause hallucinations, then hallucinations of persons *should* be relatively more frequent than other hallucinations, because the causation by the real outer object would be simply added, for this class alone, to the random inner causes that produce hallucinations in general. Similarly if the deaths of persons do not tend to cause hallucinations of those persons, the hallucinations of the dying should be *no more* frequent among hallucinations of persons than the dying themselves are frequent among persons; whilst if on the contrary the dying, and the dying alone among persons, do cause hallucinations of themselves, then these hallucinations should be more frequent among hallucinations of persons than the dying are among the whole population of persons. This latter ratio is what the Sidgwick committee finds realized in fact; hence its conclusion that the dying do cause hallucinations of themselves. Herr Parish's selection of the total number of hallucinations *überhaupt* as one subjective term of comparison leads to a statistical test which is also true in theory, *provided the corresponding objective terms be altered to match.* We shall then have (if dying persons do *not* cause hallucinations of themselves) this proportion: As is the ratio of real dying persons to all other real things, so at its highest should be the ratio of hallucinations of the dying to all other hallucinations whatsoever. But although there is no theoretic objection to this proportion, it is practically worthless, because we have no statistical data by which to compute the ratio of dying persons to all other real things.

the earlier part of their lives as if, in spite of their yielding smaller 'returns,' they must really have included as large a number, proportionally, of similar experiences. Taking the past 3 months as the trustworthy period, and considering visual cases alone, the authors of the report agree that the face-returns should be multiplied by 4, in order to represent the true number of 'apparitions' seen by their informants. But, as the total number of specifically described apparitions of recognized living persons returned in the census equals 350, and 350×4=1,400, the round number of 1,300 may be taken as probably near the figure sought.[1]

The whole number of death-coincidences amongst the 350 cases in question is 65, or 62 when 3 cases known to be selected by their collectors are struck out. There is no ground for supposing that death-coincidences tend to be forgotten by their percipients: On the contrary the cases appearing in the census date with disproportionate frequency from by-gone decades. This, of course, may be due to the fact that the number 62 is too small to give true averages when distributed over the 36 years covered. But to be on the side of severity the committee assume that the proportion reported from the last decade is the only normal one, and that the earlier stories may be false, and (by a computation based on figures which need not here be reproduced) they knock off 22 on this account from the total of death-apparitions to be used, and make it 40 instead of 62, just the opposite treatment to that which they applied to the gross group of 350 cases of which these death-cases are a part. From these 40 they again knock off 8 as an ample allowance for possibly unreported selection on the collector's part[2], and again 2 for good measure and as a sop to the adversary, so that finally the reduced number of 'veridicals' to be compared with the augmented number of veridicals and non-veridicals taken together, falls to the figure 30 which is used in the conclusion quoted from the report on a previous page.

[1] The period of three months is found trustworthy when 'suspicious' cases are eliminated. Suspicious cases are those where the appearance may not have been an hallucination. Figures seen in a bad light, or through an open door in passing, or at a distance in the open air, are included in this category. Study of the cases reported to have occurred within three months of the accounts given, shows that these 'suspicious' ones are rarest in the first month, and are therefore presumably peculiarly liable to oblivescence. But if they are counted in, one month and not three months becomes the trustworthy period, and the multiplier of the crude returns must then be changed from 4 to 6½. The influence of this counting of suspicious cases is considerably to enlarge the total of hallucinations to be supposed, and to make the odds in favor of the coincidental ones being due to something else than chance sink from 440 to 292 against 1.

[2] The data for computing this number of 8 are given on p. 243 of the report.

The reader will appreciate the candor of the committee, and see how earnestly they have sought to eliminate all that might add specious color, as distinguished from real weight, to their own side. The reader whom their argument does not impress will have, they say, to take one of three courses. He may deny the accuracy of the coincidental cases, to which the reply of the committee consists in printing 31 good ones as a sample. He may still insist that the collectors have loaded their returns with an excessive number of these cases, to which the reply is too minute for quotation here (pp. 57 and 210 of the Report) but amounts to a detailed proof that there is probably no overloading of the returns in general with *yeses*, and to good reason shown for the opinion that of the 62 coincidental apparitions taken as a basis for the enquiry, at most 10 can be assumed as possibly added deliberately by the collectors to their returns. But these have been eliminated in the reduced number of 30, finally admitted to count in the argument.—Thirdly the objector may say that many of the veridical apparitions *are* causally connected with the death, but not by telepathy or any other *vis occulta*. The illness of an aged person is the cause both of death and of anxiety among relatives. Anxiety is proved by the committee's own facts to predispose to hallucination[1]; so both the hallucination in such cases and the death can be common effects of a single natural cause, the illness, working on two persons. This, it will be remembered, is Parish's final objection, mentioned above; and the report treats it as important. At the same time the authors point out that there are but 23 cases of the 62 veridicals in which the illness was known beforehand, and only in some of these was there anxiety. Moreover the close coincidence *in hour* of the death with the apparition in so many cases seems to preclude the application on a large scale of a cause like anxiety which in the nature of things must have lasted many hours or days.[2]

[1] Anxiety about illness was probably present in 89 out of the 1622 cases of which there are first-hand accounts, and grief about death in 42 of the other cases, making nearly 1-12 of the whole number. As we don't spend 1-12 of our lives in grief and anxiety of these sorts it must be that during these emotions hallucinations come with undue frequency.

[2] Mere expectation, which often causes illusions, seems to play no important part in causing hallucinations. At least the committee find only 14 cases in the whole collection where the phantasm was of a person for whose arrival the percipient was looking out. They give cases where 'suggestion' may be reckoned a cause (collective cases, prediction of apparition at spiritist seance, etc.), but these are ambiguous, and if occult agency be once admitted as a possibility, are perhaps as likely to be caused by that as by 'suggestion'.

It will thus be seen that the committee have considered on their own account all the difficulties urged by Herr Parish (with the exception of the 'pseudo-presentiment' hypothesis of Royce) and that they have considered them in a more objective and less conjectural way than he, without their case being weakened to any certain extent.[1] Plainly, though, if the 30 cases left to be used in the argument could all have been first-class cases (with record of hallucination before event, no anxiety, etc.) the argument would have been more convincing. But the successive weedings of the crude number 62 could not be performed selectively so as to accomplish just this result, and the Census is therefore still too small for *knock-down* proof of occult cause. If telepathy be regarded on other grounds as possible, then these statistics make it extremely probable. Otherwise they will not convert the disbeliever, who will pooh-pooh the statistical method *in toto* when it takes 17,000 answers to get 30 good cases to cipher with, saying that the field is too vast and lean for profitable reaping, that figures got by applying so many hypothetical corrections to inaccurate crude data, savor too much of guess-work to inspire confidence, and that cooked returns are cooked returns, even though, like these, they be cooked for the safe side, the side adverse to the conclusion reached by their means.[2]

This sort of reception by the hard-hearted is inevitable, and it is useless to ask how strictly logical it may be, for belief follows psychological and not logical laws. A single veridical hallucination experienced by one's self or by some friend who tells one all the circumstances has more influence over the mind than the largest calculated numerical probability either for or against. I can testify to this from direct observation. The case will, therefore, still hang

[1] The only criticism I can make is that the committee have possibly been too indulgent to the cases where the percipient was in bed. His conviction that he was awake is to be taken with large allowance under these circumstances.

[2] The figure 4, for example, used as a multiplier of the crude returns in correction of forgetfulness, is reached by this process: out of 87 visual hallucinations reported for the most recent year, 42 are stated to have occurred within the most recent quarter, and of these 19 within the most recent month, and 12 within the most recent half-month; numbers which correspond approximately to 168, 228, and 288 per annum instead of 87. But if from the 87 the 'suspicious' cases as described above are eliminated, and the most recent quarter examined, the figures are much more even. There are 12 suspicious cases in the recent quarter; so that then 30 instead of 42 becomes the number to be counted in the quarter. Of these the last month shows 12, and the last half-month 5, numbers which correspond to 120, 144, and 120 per annum respectively. This looks like distribution by 'natural law,' *provided the evenness of the figures be not accidental.* But where such small numbers are involved, how can one be sure on that point?

pending before public opinion, in spite of the laborious industry of Mrs. Sidgwick and her colleagues. Of course if the results of the American Census, not yet published, should correspond, that will add retroactive weight. But the most that can be said, so far, in the opinion of the present writer, is this, that the Sidgwick report affords *a most formidable presumption* that veridical hallucinations are due to something more than chance. Now this means that the telepathic theory, and whatever other occult theories may offer themselves, have fairly conquered the right to a patient and respectful hearing before the scientific bar; and no one with any real conception of what the word 'Science' means, can fail to realize the profound issues which such a fact as this may involve.

WILLIAM JAMES.

ETHICAL.

A Study of Ethical Principles. JAMES SETH. New York: Imported by Charles Scribner's Sons. 1894. Pp. XVI, 460.

The subject is presented in three parts;—(I) an analysis of the psychological basis of ethical principles, and criticism of the corresponding systems; (II) a discussion of the virtues, under the caption of The Moral Life; and (III) the metaphysical implications of ethics. In Part I, Prof. Seth criticises Hedonism as unduly emphasizing the sentient nature of man on the one hand, and so-called Rigorism on the other as laying exclusive stress upon man's rational nature. Each is based upon a partial psychology, and hence incomplete and misleading. He would therefore distribute the emphasis, so that the total personality embracing both sensibility and reason is regarded as the proper basis of ethical principles. This personality differs from the lower, or animal self-hood of mere individuality in the power of transcending the entire impulsive and sentient life, subduing it unto the higher rational self. This power constitutes the will, and differentiates man from the animal. Following the epistemological analogy, as the Ego constructs the various data of sensation through the apperceptive process, forming out of them an object of knowledge, so in the construction of the moral end out of the impulses, there is a similar synthesis of the crude data of sensibility. Prof. Seth's ideal, therefore, is self-realization, and his ethical system he styles Eudaimonism, wishing to restore its original Aristotelian significance which presented pleasure as 'the very bloom and crown of goodness.' We question the propriety of using

a word so strongly associated with Hedonism, to characterize a system which subordinates the pleasure elements to a superior ideal. It seems more appropriate to classify Prof. Seth among the following of Hegel and Green, on account of the prominence of the idea of self-realization in his scheme. He, however, contends that they have under-estimated the sharpness of the existing dualism, in affirming the essential rationality of the life of sensibility; and that the full force of the antithesis must be appreciated in order to realize that complete synthesis where inclination and duty are one.

Prof. Seth criticises Rigorism as presenting an abstract formal law of conduct, which, however, is devoid of any definite content. But does he himself escape a like imputation?—inasmuch, as since his ethical ideal is the realization of self-hood, or personality, the question naturally arises, what self? What kind of personality? It is the self constituted by the common rational Ego in each individual. But such an ideal is formal, and as truly lacks a definite content, so that we are confronted by the old difficulty in a new form. In Part III, the metaphysical implications of ethics, the author treats of freedom, God and immortality as necessitated by his doctrine of personality. Since self is more than the sum total of sensations, it is so far forth superior to the sensuous stream of consciousness, and therefore free. Also self seeks a larger environment than Nature, that is God; whence immortality is naturally deduced. This also furnishes a supplement to the Kantian theory of Autonomy, inasmuch "as the moral law is the echo within our souls of the voice of the Eternal, whose offspring we are."

The Kantian Epistemology and Theism. C. WISTAR HODGE. Philadelphia, MacCalla & Co., 1894. Pp. 47.

The author criticises Kant's epistemological position, because he does not follow it out to its logical conclusion. Kant's primary presupposition that all things exist only in relation to self-consciousness necessitates a second presupposition deducible from it, namely, that the real is the rational. This is overlooked; and the spirit of Kant's teaching as well as the logic demands its recognition. Dr. Hodge contends that the ideas of reason must be more than mere logical universals; that there is a necessary and vital connection between knowledge and being within our consciousness, that the activity of the mind necessitates its universality and reality; that there is a unity of organic experience, and an objectivity of the categories. By an acute analysis of Kant's position, he seeks to prove the necessity of these supplementary propositions, especially as throwing light

upon Kant's theistic criticism. He shows that there is need to emphasize God's immanence as well as His transcendence. The two ideas are not mutually exclusive but are realized in one self-conscious and personal spirit; and Kant's criticism of theistic arguments proceeds upon the basis of a transcendent Being merely, the mechanically conceived God of Deism. Finality also must be conceived as an objective fact; and this strengthens the reasonableness of the theistic position. Moreover the Kantian ethics in relation to theism, overlooks the fact that the self-revealing spirit can be like us because we are formed in His image; and if our noumenal self carry with it a moral ideal, so must God also be conceived as possessed of moral attributes.

The Æsthetic Element in Morality and its Place in a Utilitarian Theory of Morals. FRANK CHAPMAN SHARP. New York, Macmillan & Co., 1893. Pp. 131.

The problem presented in this work is to determine the place of beauty of character in the moral world. Dr. Sharp approaches this problem from two different sides. First as to its origin, he contends that every attempt to make beauty of character the primary product of the moral forces is doomed to failure, and he insists that their original goal is the general happiness according to a utilitarian criterion of right and wrong. In the elaboration of this, he shows affinity of thought with Shaftsbury and Hutcheson. From a second point of view, he discusses the question of values, and reaches the conclusion that beauty of character is but one of the many sources of æsthetic emotion, and that its attraction for us is due to the pleasure it affords, the worth of which is to be measured by the same scale which we apply to the other emotions. In all this, the author seems to us to be himself alive to his omission of the idea of obligation as an essential factor. For he supplements his discussion by an analysis of the idea of oughtness. This is for him in the main, the pressure of the accumulated judgments of society upon the individual consciousness. This overlooks the principle of autonomy; and man's will then feels obligation only as imposed from without. This is especially unsatisfactory where Dr. Sharp speaks of the 'theological ought.' He regards God in the same manner as society, or our fellow men in general, one of the powers without, which expect from us certain lines of conduct. Such a view makes obligation unreal, by presenting it as a force both arbitrary and artificial.

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NEUROLOGY.

Ueber die Histogenese der Körner der Kleinhirnrinde. ERNST LUGARO. Anatomische Anzeiger, No. 23, 1894.

Recent studies in the development of the nervous system have brought to light some interesting and curious migrations of nerve cells. His showed that the neuroblasts arising from germinal cells about the central canal must migrate to the different parts of gray matter in which they underwent their further development. The migration of a spherical cell probably possessing amœboid powers is familiar enough from the manner in which the leucocytes travel through the fixed tissues. But in some way nerve elements, if the interpretations given are correct, must traverse the tissues even after their prolongations have been formed.

The paper in question is a research on the granules or small cells of the cerebellum. On the surface of the developing cerebellum is a layer of epithelium like cells with one process passing directly towards the surface and the other more or less in the opposite direction. These elements shade off by intermediate forms into dineuric cells, the two neurons of which run parallel with the cortical surface. Among those lying somewhat deeper, appear dineuric elements from which a dendritic process has begun to grow away from the surface. Just as in the case of the dineuric cells of the spinal ganglia, the neurons come to lie more and more at one side of the cell body; finally a stem is formed and this stem lengthens, the cell bodies sinking deeper into the granular layer. The dendritic processes appear first as a single conical outgrowth, then become branched, multiple and shorter, and finally decrease in number with the formation of the characteristic brushes at their termini and become identical with the cells of the fully developed granular layer, the very long stem of the neuron passing into the molecular layer and terminating in the T process now so well known from the studies of Cajal, Köl-liker and others.

If this is a true history of the development of these granules, the author is justified in his conclusions: (1) The position of the nerve cells durlng embryonic life is not necessarily that which it will finally hold. (2) The neuron grows not only at its ends but also throughout its entire length, thus permitting the cell body to wander. (3) While the neuron undergoes a gradual and progressive development, the dendritic processes may at some intervening time be better developed than they are at the end of growth.

The disappearance of the primitive and intermediate forms as the

final form becomes more abundant, is one of the arguments in favor of the author's view; but there is no escaping the conclusion that not only the cell-body with its dendrons must then move through the surrounding substance, but the neuron with its termination also moves. While therefore the argument in favor of these changes appears complete, it requires us to admit that the nerve cell with all its prolongations may, during growth, sink through the substance of the cerebellum, like a bullet through a plate of wax.

Although these experiments do not stand alone, nevertheless so difficult is it to accept this explanation that careful control observations on displacements due to the enlargement of other nerve structures should be made before entertaining too seriously the conclusions to which these results apparently point.

Beitrag zur Kentniss der Pathologischen Anatomie der Paralysis agitans und deren Beziehungen zu gewissen Nervenkrankheiten des Greisenalters. EMIL REDLICH. Jahrbücher f. Psychiatrie, Bd. XII. Heft 3.

In connection with the changes occurring in the nerve cells in old age, the observations on the nervous system in paralysis agitans are of importance. The paper by Ketscher, 1892, on this subject, indicated that the appearances found in the nervous system of the aged suffering from paralysis agitans were in kind similar to those found in persons who, though aged, did not exhibit this disease. The difference between the appearances in the two sets of cases were in his opinion mainly one of degree.

In view of Ketscher's publication, Redlich gives his results in a condensed form, as they are mainly confirmatory, though in part divergent. In paralysis agitans the spinal cord is especially affected, and in it the lumbar and cervical enlargements are the centres of greatest change. Here the most marked alterations are in the lateral and dorsal columns where there is a sclerosis with atrophy of the nerve fibres, vascular changes with the formation of amyloid bodies, while the cells in the ventral horns and the columns of Clarke are often so pigmented as to obscure the nucleus. In other portions of the central and peripheral system similar changes occur, though they are less intense.

Redlich urges a sharper distinction between the condition of the central nervous system in uncomplicated old age and those found in paralysis agitans. He would separate the pathological anatomy in these latter cases from those of simple senility by the greater changes in the blood vessels and formation of sclerotic areas due to the in-

crease in the supporting tissues, changes which do not necessarily accompany the involutionary process in the central system.

Histological changes induced in sympathetic, motor, and sensory nerve-cells by functional activity. (Preliminary note). GUSTAV MANN. Jour. of Anat. and Physiol., N. S., Vol. IX., Part I., October, 1894.

Previous to this publication, there have been current two statements concerning the effect of electrical stimuli on the size of nerve-cells and their nuclei. Hodge, working with weak faradic currents, applied intermittently, but for a long time, to the sensory spinal nerves of the frog and cat, found the cells of the spinal ganglia thus fatigued, to be shrunken. Vas, with stronger stimuli applied for fifteen minutes to the trunk of the cervical sympathetic of the rabbit, found the bodies and the nuclei of the sympathetic cells thereby swollen.

To the examination of this discrepancy the author has directed his observations. He repeated Vas' experiments under his conditions, and obtained similar results; a swelling of the cell-body, its nucleus, and the structures within the nucleus. On examining parts of the nervous system of a dog after prolonged muscular exercise, the motor cells in the lumbar region of the cord were found to exhibit the characteristic shrinkage described by Hodge, while cells from the motor region of the cortex showed swollen bodies and nuclei.

When the two retinæ of a dog were compared, one retina having been at rest while the other had been stimulated, both results were obtained within the limits of the same retina; the nuclei of the rods showing decided shrinkage while those of the middle ganglion-cell layer were swollen.

These differences lead the author to suggest that *activity* in nerve cells is accompanied by an increase in volume, whereas *fatigue* is associated with the reverse change. These terms are not happily chosen, since both apply equally well to the entire series of changes taking place in the cell. With the commencement of activity, fatigue commences and they continue together as different aspects of a single process. It would therefore be better to distinguish between the earlier and later phases of one or the other. In the early stages of fatigue, associated with enlargement, M. finds that the cell, and especially the nucleus, become chromophobic, and it is interesting to note that in his study of the mature cells, in the cervical enlargement of the mammalian cord, Kaiser found the chromophobic cells to have the greater mean diameter.

Like all such matters the histological changes accompanying the functional activity of nerve cells, becomes more complicated, the further it is examined, and it is to be hoped, that it will in this instance be possible to unravel these apparently opposite reactions without any obfuscation of the main point, due to the neglect of the mechanical conditions of experiment, or of the full record concerning the species, age, sex and physiological condition of the animals employed.

Quelques observations expérimentales sur l'influence de l'insomnie absolute. MARIE DE MANACEÏNE. (Address made before the International Medical Congress held at Rome, 1894). Archives italiennes de Biologie, 1894. T. XXI.

Dogs from 2 to 4 months of age were prevented from sleeping and the physiological and anatomical effects of this treatment, recorded. It appears that, for these animals at least, loss of sleep is much more detrimental than starvation. Dogs after starving more than twenty days and having lost more than 50 per cent. of their initial weight, may still recuperate under favorable conditions; but loss of sleep for four or five days is fatal. The temperature of the sleepless animal finally falls as much as 8° below the normal, the reflexes disappear, the red blood corpuscles first diminish in number, to undergo a final increase during the last two days, when the animal refuses food.

Fatty degeneration of the tissues was the chief histological change noted at the post-mortem examination; the blood vessels often appeared compressed, were surrounded by leucocytes and there were capillary hemorrhages on the surface of the cerebral hemispheres, with more extensive ones along the optic pathway; while the spinal cord appeared abnormally dry and anæmic.

When it is remembered that the central system withstands the effects of starvation in a most remarkable manner, maintaining almost its full weight up to the death of the animal, the great disturbance following a few days' loss of sleep is very impressive.

Zur Kenntniss der Veränderungen des Rückenmarkes beim Menschen nach Extremitätenamputationen. A. GRIGORIEW. Zeitschr. f. Heilk. 1894. Bd. XV.

The physiological conditions on which a mature nerve cell depends for its healthy maintenance are complicated. Most important are the supply of nutritive substance; the regular alternation of ac-

tivity with repose and anatomical completeness; but the essential feature in each one of these conditions is far from clear.

G. has examined the spinal cord in the five cases of amputation, two at the upper arm, two at the thigh, one below the knee. The last lesion was but a year old when examined, and the cord showed no changes. In the other four cases there was an atrophy on the corresponding side of the cord, noticeable earliest in the dorsal roots and dorsal columns, later in the cells of the ventral horns. The degree of the atrophy increased with the age of the lesion and was accompanied by some degeneration. These results are mainly confirmatory of previous observations. On attempting to analyze them it will be readily seen how complex they are.

In these cases both the sensory and motor elements have lost portions of their neurons without an opportunity to regenerate them. The sensory elements have been deprived of the major number of stimuli coming to them under normal conditions, and have thus failed to send the usual impulses on to the cord. The motor elements have been crippled by the loss of their peripheral outgrowths and allowed to become torpid by disuse. There are, to be sure, other impulses coming to these cells, in addition to those arriving by their associated dorsal roots, and they must, for some time at least, retain the power to discharge along their efferent prolongations. To explain this atrophy in the cord after amputation, it will therefore be necessary to learn what atrophic changes occur, when either root, a dorsal or ventral, alone is cut and for what period after operation the cell bodies continue to send out impulses.

Note on the Degenerations following Double Transverse, Longitudinal, and Anterior Cornual Lesions of the Spinal Cord. ALBERT S. GRÜNBAUM. Journ. of Physiology, Vol. XVI.

On isolating by double transverse section about three spinal segments in the lower thoracic region of the cord of a monkey, it was found that there followed a very complete degeneration of the dorsal columns, and in the remaining columns a sharply defined peripheral band marking out the distribution of the long tracts at the circumference. The degeneration increased with time, being greater at the end of the six months than at the end of one. After this lesion the included anterior root fibres do not show degeneration, indicating that they rise within the limits of the portion isolated, but when the ventral cornu of one side has been destroyed, some degenerated fibres are to be found in the opposite ventral root (observations on cats). A longitudinal mesal section of the cord was made (also on

cats) with the result of an ascending degeneration just external to the dorsal roots, and another at the ventral border of the cerebellar tract. This latter is explained as due to the section of fibres crossing in the anterior commissure, while the former is attributed to injury of the columns of Clarke.

Report of the Versammlung deutscher Naturforscher und Aerzte in Wien vom 24–30 September, 1894; *Section für Psychiatrie und Neurologie.* Neurologisches Centralblatt, No. 20. 1894.

From the reports of a number of papers of neurological interest, given at the Vienna meeting of the German Society of Naturalists and Physicians, the following abstracts are taken:

Ueber die topographischen Beziehungen zwischen Retina, Opticus, und gekreutzen Tractus beim Kaninschen. A. PICK.

From the destruction of limited areas in the retina, and the study of the subsequent degeneration, it appears that the affected fibres occupy in the cross section of the opticus or tractus a position corresponding to that of the injury in the retina. No allusion is made to facts bearing on the bundle of uncrossed optic fibres, though there is reason to think that in the rabbit this is connected with the lateral portions of the retina.

Die Sinnesorgane und die Ganglien bei Anencephalie und Amyelie. O. v. LEONOWA.

In a fœtus of eight months in which the medullary tube was lacking, the author had previously shown the presence and moderate development of the sensory system. In this new case, also a fœtus of eight months, anencephalic and amyelic, the nervous system consisted of the retina and of spinal and sympathetic ganglia, with the nerves arising from them. A portion of these nerves could be followed into the muscles. The spinal ganglia were not completely separated from one another, but the dorsal roots had been formed and, except in the cervical region, these roots ran cephalad. The nerve cells were complete; some of the fibres medullated, but in the so-called retina there were no nerve cells, neither were there any fibres in the optic stalk. The independent development of the sensory system, the sympathetic being associated with it, the distribution of sensory nerves to the muscles, and the normal development of the musculatur, are the striking features of this case. The last fact is very interesting, for in normal persons the severance of the ventral roots from the muscles leads to degenerative changes in

the latter, and the fact that the muscles can develop without such a nerve supply is most remarkable.

Ueber die feinere Anatomie und die physiologische Bedeutung des sympathischen Nervensystem. V. KÖLLIKER.

The medullation of sympathetic fibres is very irregular, and they exhibit every combination which can occur. K. considers the functions of the sympathetic system partly independent—an expression not explained—and partly dependent on the central system. The cells he describes as mononeuric. Those cells with a spiral and straight fibre give origin only to the latter, while the former originates elsewhere and merely terminates on the body of the cell. In the mammals these sympathetic cells may be multipolar. In this latter case, the dendrons are pathways for afferent, and the neuron, for the efferent impulse.

In this view the cells are mainly efferent or motor, while the afferent impulses, passing first to the medullary centres, are mediated by a few fibres from the dorsal spinal roots. It is not easy to see how independence can in any sense be granted to this system, unless the anatomical arrangements for both afferent and efferent impulses are present in it. It is suggestive to recall, moreover, that the most probable point of origin for the sympathetic cells is the same as that which gives rise to the cells of the spinal ganglia, which are the typical sensory elements. H. H. D.

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VISION:

OSCILLATIONS IN THE RETINAL PROCESS.

(*1*) *Ueber die nach kurzdauernder Reizung des Sehorgans auftretenden Nachbilder.* CARL HESS. Pflüger's Archiv. XLIV, 190. 1891.

(*2*) *Ueber Nachbilder.* SNELLEN. XXIII. Vers. ophth. Gesellsch. zu Heidelberg, 1893.

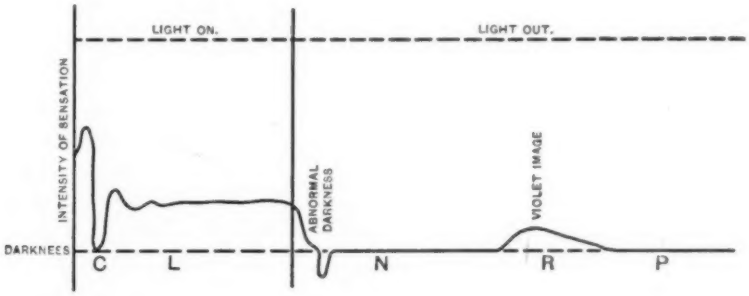
(*3*) *Primäre, secundäre und tertiäre Netzhautbilder nach momentanen Lichteindrücken.* H. P. BOSCHA. Arch. f. Ophth. XL, (1), 22–42, 1894.

(*4*) *Studien über Nachbilder.* CARL HESS. Arch f. Ophth. XL, (2), 259–279, 1894.

(*5*) *On the Recurrent Images following Visual Impressions.* SHELFORD BIDWELL, Proc. Roy. Soc. LVI., June 7, 1894.

Of these several papers on after-images, the last is the most important. The experiments of Mr. Bidwell (like those of Hess in his

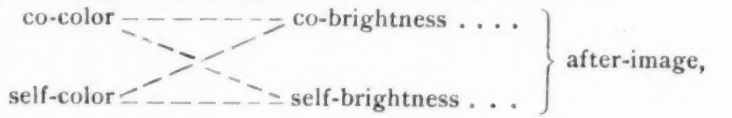
second communication) concern the phenomena which arise when bright colorless or colored objects are made to move rather rapidly before the eye in an otherwise dark room. Hess used small glow lamps of .5 cm diameter at a distance of about half a meter from the eye, covered with glass of different colors. Bidwell found colored glasses quite inadequate to his purpose, and hence made use of homogeneous light, obtained by a high pressure oxyhydrogen light, a bisulphide of carbon prism, a screen with a slit in it, and a mirror which rotated about a non-perpendicular axis and so caused the portions of the spectrum reflected from it to describe a circle upon a second screen. With various modifications of this apparatus he obtained a complicated series of sensations which will be best borne in mind if we reproduce his diagram :



Immediately upon the impact of the light there is experienced a sensation of luminosity, the intensity of which increases for about one-sixtieth of a second. Then follows suddenly a sensation of darkness, lasting also for about one-sixtieth of a second ; this is the now well-known Charpentier oscillation. Several slight waves follow this, and then there is a period of steady luminosity, which, however, is much less intense than the first instantaneous maximum. Upon shutting off the external light, a sensation of diminishing brightness continues for a brief interval, and is followed by a "sudden and clearly-defined sensation of what may be called abnormal darkness—darker than common darkness—which lasts for about one-sixtieth of a second," and is followed by another interval of ordinary darkness (N). Finally there occurs another transient impression of luminosity, *generally violet-colored* (R). This is the Recurrent Image, which is the special subject of this paper. The recurrent image is followed by what the author speaks of sometimes as a period of steady darkness—" after which the uniformity of the darkness remains undisturbed" (p. 143)—but at other times he re-

fers to it as a phosphorescent trail, the color of which cannot be determined, but which is with some observers "so intense that the recurrent image cannot be distinguished from it at all." This whole period (R and P) is what observers in general put under the head of the positive after-image; it has almost always been supposed to be continuous with the original sensation, and its color (which according to all observers is most persistently a reddish violet) is what Helmholtz describes as *farbiges Abklingen des positiven Nachbildes*, a term which will no longer be appropriate. The fact that the positive after-image is preceded by a negative phase (N) has recently been made plain by Hess (1), who points out that it has been wholly overlooked by Helmholtz, Aubert and Fick; it had, however, been distinctly described by Purkinje, whose works are still a store-house of facts which await a sufficiently careful observer to be rediscovered. Bidwell, strangely enough, wrote in ignorance of this paper of Hess, and Hess was also unaware that the interval of darkness which interrupts the positive image had already been studied by Prof. C. A. Young and by Mr. A. S. Davis (Phil. Mag., Vols. 43 and 44, 1872). The fact that when the after-image of the whole spectrum is formed, its brightest part is not in the yellow but in the green (Hess and Bidwell) will doubtless be found to connect itself with the fact that the green is the brightest part of the spectrum when seen in a faint light.

All recent work in vision shows that it becomes more and more necessary to distinguish between the specific sensation (color) and the absolute sensation (brightness) produced by objective light, and in particular the terms positive and negative as applied to after-images are very misleading unless farther particularized, for an image may be positive in one respect at the same time that it is negative in the other. It is, for instance, on account of this ambiguity, an almost hopeless task to endeavor to compare the phases of the after-image as described in the five papers whose titles are given above. In order to secure comprehension and at the same time to avoid the use of such complicated phrases as "an after-image which is complementary to the original one in color but of corresponding brightness," I would propose, in the absence of a less awkward terminology, the following; *co-color*, as an abbreviation for complementary color; *self-color* for a revival of the same color, and the same prefixes for the phases of brightness. Thus we should have:



for the four possible descriptions of after-image, and for the above long phrase we should substitute the *co-color self-brightness image.*

Boscha (3), following out the preliminary work of Snellen and in his laboratory, used an electric spark in a dark room to illuminate colored papers. His secondary stage is one of co-color and self-brightness, and it is followed, after a pause, by an image which is always of a peculiar reddish color difficult to define. He makes a number of criticisms upon the work of Hess in (1), which are shown in (4) to be based upon misconceptions. The dark interval preceding his secondary stage he seems to have quite overlooked. The conclusion of the whole matter is that there are numerous oscillations in the chemical process which is set up in the retina by light, whose farther more detailed study may be expected, perhaps, to throw additional light upon rival theories.

COLOR BLINDNESS.

(1) *Physiologische Analyse eines ungewöhnlichen Falles partieller Farbenblindheit.* II. M. v. VINTSCHGAU. Pflüger's Archiv, LVII. 191–307.

(2) *Ueber einen Fall von Gelb-Blaublindheit.* EW. HERING. Pfl. Arch. LVII. 308-332.

Prof. Langley has lately been so fortunate as to be able to perfect his methods for the determination of the cold lines in the infra-red part of the spectrum to such an extent that work which it formerly took him a year to finish he can now do in a fraction of a day, and he has already laid down 2,000 lines with far greater accuracy than he could otherwise have obtained them with at the end of a hundred years. A somewhat similar change has been effected in our power to determine the exact nature of a given case of color blindness by Prof. König's latest form of the Helmholtz color-mixing apparatus, or spectro-photometer for colors (an improvement even over the instrument exhibited at the Chicago Fair), when manipulated with all the precautions and the corrections which are made use of in König's laboratory. If v. Vintschgau had been willing to make use of this method, he might have described his case of dichromasie not in one hundred and sixteen pages, but in three, one of which would have contained two diagrams, one giving the patient's brightness curve through the spectrum, and the other giving the curves for his two color sensations, as deduced by the relative amount of the two end-sensations necessary to match in tone and brightness all the intermediate parts of the spectrum. This

simple information we seek in vain in v. Vintschgau's pages, but we have instead endless series of experiments with colored paper, glass, and wools, which all belong to a previous period of color-blindness investigation, and which simply repeat the result obtained by his spectrophotometer,—namely, that the patient lacks the sensations blue and yellow, as in a typical case of blue-blindness, and that he has a slightly diminished sensibility for the colors that remain to him, for the short-wave end of the spectrum rather more than for the long-wave end. This is hardly enough to make the case 'unusual,' and Hering refers to it simply as *a case;* the adjective in v. Vintschgau's title is, in fact, simply a survival from his first communication, when he had convinced himself, also by an immense number of experiments, that his patient could see yellow, which would indeed have been an anomaly. This mistake he was led into by paying attention to the *names* which his patient used for his sensations. How wholly unjustifiable this is was pointed out by Dr. William Pole, himself partially color-blind, in an article of most remarkable logical acumen, in which the writer wholly anticipated the facts in regard to color-blindness which have only with the last few years gained acceptance (*Trans. Roy. Soc.*, 1858). The violet end of the spectrum gives this individual no color-sensation when looked at directly (2), but it has been possible to prove indirectly, and in three different ways, that he has here a sensation of red; (*a*) a little of this light when added to a red which is too faint to be distinguished causes a plain sensation of red; (*b*) it forms a colorless mixture with green, and (*c*) it gives a distinct green as a contrast-color. This fact that a color too faint to be perceived may yet cause its proper contrast effect to be very distinct is already known, and is beautifully exhibited in the experiments with a projection lantern of Rollet (Pfl. Arch. XL. 25, 1892,) the most brilliant yet made in the subject of contrast.

The necessity of paying no attention to what the patient says that he sees, but only to his color equations, is now pretty well recognized, and Hering himself (but not v. Vintschgau, p. 205) distinctly insists upon it. Nevertheless, in calling the two color-sensations of the person here investigated red and green, Hering (and v. Vintschgau as well) overlooks this precaution, and at the same time assumes the correctness of his own theory, which the case is supposed to support. All that we know is that this patient has some distinguishable color-sensation along the spectrum as far as λ 596, and again another sensation from λ 574 to λ 481. *If Hering's theory is correct*, this latter sensation is green; upon the Helmholtz theory, as

held by its present defenders (and upon any three-color theory), it is blue-green; the whole bearing of the case upon existing theories depends upon whether this color (the complement to the fundamental red of both theories) is properly named green or blue-green; in other words, it has no bearing at all upon them. König has very recently had several most interesting cases of monocular blue-blindness circumscribed within a small area surrounding the fovea, in which the sensation given by the cold end of the spectrum can be readily compared with normal sensations, and is found to be blue-green. This fact is still, of course, not decisive between theories, for what Hering means by green is a color which is pronounced to be blue-green by the ordinary consciousness, and this in spite of the fact that it is precisely for the satisfaction of the ordinary consciousness as regards color-sensation that Hering's theory has been devised.

C. LADD FRANKLIN.

SENSATIONS OF TASTE.

Contributions to the Physiological Psychology of the Sense of Taste. FRIEDRICH KIESOW. Abstract of papers in Philosophische Studien, Bd. X, Heft 3, pp. 329 ff; Heft 4, pp. 523 ff.

The electric, metallic and alkaline tastes being reserved for special investigations, the present work treats only of those sensations recognised as special qualities, viz., sweet, sour, salt and bitter. The taste-substances used were: chlornatr., muriatic acid, sacch. alb., sacch., quin. sulph., and quin. pur. In all cases in which chemical combination of the substances was to be strictly avoided quin. sulph. and not quin. pur. was used. The greatest possible chemical purity was sought for these substances, which were dissolved in distilled water. The application was made partly by means of dropping-glass tubes on which a scale graduated by $\frac{1}{10}$ cm. was engraved, partly by means of soft pointed hair brushes. All disturbing accompanying sensations, not excepting that of temperature, were excluded. The simplest way to accomplish the last was to raise the fluids to be applied to the temperature of the mouth, viz. 37° C. Between the separate experiments the mouth was rinsed out with pure water of the same temperature, 37° C. After having trained the subjects, I first examined the cavity of the mouth, with a view to determining what parts were receptive of sensations of taste. These experiments were performed both on children and on adults. Taking into consideration what former investigators have found out the total results of this chapter will be given.

1. Besides the whole surface of the tongue together with its base and the under surface of its tip—the hard and soft palate, without doubt the arcus glosso-palatinus, the tonsils, the uvula, the isthmus fancium, the inside of the epiglottis and the mucous membrane of the cheeks participate in the sensation of taste.

2. All these parts are sensitive in childhood; in adults the mucous membrane of the cheeks, the middle of the tongue and, with a few exceptions, the hard palate lose their sensitiveness. In some cases the under surface of the tip of the tongue on both sides of the frenulum remains receptive also in adults.

3. The presence of disturbance is accounted for, sometimes by an affection of the cavity of the tympanum, sometimes by individual differences.

It must be remarked that the perceptive faculty of the inner epiglottis was established by Michelson and Langendorff,[1] that of the mucous membrane of the cheeks in childhood by Urbantschitsch.[2] Concerning the retrogression of certain taste surfaces in adults I must refer the reader to my longer article in which an explanation according to the theory of development is offered and literary references given.

In a further investigation I tested the sensitiveness of the different perceptive parts of the cavity of the mouth, by taking as measure the absolute given for the different qualities of taste, obtaining in this way the following general results:

1. Sensitiveness varies for the different qualities on the different parts of the tongue. Sweet is tasted best on the tip of the tongue, sour on the edge, and bitter at the base, acid equally on the tip and edges, but less at the base.

2. With regard to the values found in an isolated case for the other taste-surfaces, the sensitiveness for sweet and bitter appears in the following order: Soft palate, arcus glosso-palatin., uvula, under-surface of the tip; for sour, arcus glosso-palat., palat. molle, uvula, under-surface of the tip; for salt, palatum molle, under-surface of the tip, arcus glosso-pal., uvula. The values are in part considerably below those noted under 1. Only on the soft palate does salt reach the normal given.

3. A single investigation showed that in childhood all parts, excepting the tip and edges of the tongue, possessed nearly the same

[1] Centralblatt für Physiol. 1892. P. 204.

[2] Urbantschitsch, *Beobachtungen über Anomalien des Geschmacks, etc., in Forge von Erkrankungen der Taukenhöhle.* 1876.—I desire to draw special attention to this interesting work.

sensitiveness with regard to sweet. The tip and edges were more sensitive.

4. The explanation of the normal condition, as of individual differences is without doubt to be found in the law of adaptation, excepting those cases in which pathological causes, obstructions, etc., appear.

Further, attention was directed to the qualitative conditions of the sensations of taste. These experiments were only made on adults. First, I was enabled to prove that all four above-named qualities are true sensations of taste, also that the sensations of sour and salt must not be excluded from the sphere of taste on account of the accompanying tactile sensations. On the contrary, my investigations led to the conclusion that all our perceptions of taste are accompanied by tactile sensations, although in different degrees. Sweet is accompanied on and near the limen by a sensation of smoothness, at higher intensity by that of slipperiness, at very great intensity by that of scratching and biting. The liminal values of bitter are accompanied by a distinct sensation of greasiness. Even the application of distilled water produced with some of my subjects a distinct perception of taste. Two of them tasted water on the tip of the tongue as sweet, on the edges as sour and sourish, at the base bitter. Others tasted it as bitter in the whole cavity of the mouth, others only bitter at the base and tasteless on the other parts of the tongue. The bitter sensation produced by distilled water accompanied the single sensation called forth by taste—substances often for a time above the limen, so that in this way two sensations arose which I have designated as double-sensations. Even a mechanical stimulus of the base of the tongue with a glass-rod produced with me and with many of my subjects a sensation distinctly bitter.

Great influence in the region of taste must be ascribed to association and the effects of contrast. The conditions of contrast I investigated with special care; the total results of which may be given concisely as follows:

1. Contrasting stimuli must be recognized in the sense of taste.

2. Salt contrasts with sweet, salt with sour, sweet with sour.

3. Salt and sweet, and salt and sour contrast both on simultaneous stimulation of corresponding parts of the tongue and on successive stimulation of the same taste-surface. The contrasts of sweet and sour could only be observed in the latter case.

4. Bitter forms an exception, but yet perhaps gives rise to contrasts restricted to individuals. THE AUTHOR.

LEIPZIG.

EXPERIMENTAL.

Psychologie des grands calculateurs et joueurs d' échecs. A. BINET. Paris, Hachette. 1894. Pp. VIII, 364.

Travaux du Laboratoire de psychologie physiologique de hautes-études à la Sorbonne. BEAUNIS, BINET and others. Année, 1892; pp. 100; année, 1893; pp. 58. Paris, Alcan. 1893–4.

Travaux du Laboratoire de psychologie physiologique pendant l'année 1892–1893. A. BINET and others. Rev. Philos. XXXVII, 111–119, 222–240 and 344–352. 1894.

The first two numbers of M. Beaunis' Annual, (*Travaux du Laboratoire,*) indicate the main lines of research that have been followed thus far in the new psychological laboratory at Paris, of which M. Binet is the leading spirit. Several of the studies appear also, as preliminary reports, in the pages of the *Revue Philosophique.* The investigations can be grouped for the most part under two general heads, *audition colorée* and memory; the latter are incorporated in M. Binet's work on *Great Calculators and Chess Players.*

M. Binet was able to secure two noted calculators and test their powers and methods at some length. One of them, M. Inaudi, was distinctly of the auditory type, preferring to receive his data orally; when required to start with written numbers, he always repeated them to himself, before proceeding with the problem. The other, M. Diamandi, a Greek, was quite as markedly of the visual type. M. Binet devotes a chapter of his work to some tests made on these two, together with a distinguished prestidigitator, M. Arnould, to determine their comparative facility in memorizing and repeating numbers. M. Arnould's method is to associate each numeral with a certain consonant; by adding vowels at will, he transforms any given number into some word or phrase, which he readily memorizes; when called on he simply translates this back into the number. M. Inaudi could memorize the most rapidly of the three up to a hundred figures, (36 in 1m 30″, 75 in 5m 30″, 100 in 12m.), which was as far as he was tested. As between MM. Diamandi and Arnould, the former learned the first few figures somewhat more quickly, but from 25 places on the latter had a decided advantage; (D: 10 in 17″, 25 in 3m, 50 in 7m, 100 in 25m; A: 10 in 20″, 25 in 2m 30″, 50 in 2m. 45″, 100 in 15m). The two 'direct' memorizers were compensated, however, by being able to *repeat* what they had learned much more rapidly than M. Arnould. The investigation is particularly interesting in view of the dispute as to the

value of mnemonics. It would be interesting to test also the comparative *durability* of the several kinds of memory by experiments similar to those of Ebbinghaus. M. Inaudi was able to repeat 230 numerals that he had learned on the preceding day, but the experiment was not followed out; it seems probable that M. Arnould would have been able to retain many times that number. (Mnemonic memorizing, which relies on mediating associations, might be called the *associative* type, to distinguish it from the auditive and visual, where the correlation is immediate.) — M. Binet endeavors to account for the rapidity with which MM. Inaudi and Diamandi perform complicated mathematical operations, in a number of ways. (1) By the use of various devices which simplify the problem. (2) By an extension of the multiplication table to products of two place numbers. (3) By the aid of unconscious mental processes: just as we unconsciously adopt results in multiplication, from the memorized table, so one accustomed to calculating might come upon results intuitively, which he would require a long and tedious operation to reach, if every step had to be formulated. Proceeding mechanically or half-consciously, he would not need to use words or symbols to indicate the operations; this abbreviation would in itself shorten the time considerably, and M. Binet agrees with Scripture in emphasizing its importance in the 'shorthand' of calculation, even if the process be wholly conscious and voluntary.

The second part of M. Binet's work treats of blind-fold chess-playing. By means of a *questionnaire* and personal letters, he obtained from a large number of players more or less complete analyses of their methods of procedure. According to this testimony, there are three principal requisites for success: (1) Familiarity with the game, through practice and study; especially a knowledge of the best line of play for various positions, in order to save time and thought. This is far more essential here than in play over the board. (2) Memory, in order to retain the moves already played or the position of the pieces at a given time. (3) Power of visualization. This last is the distinguishing feature of blind-fold playing. In some players it takes a very concrete form: they seem to *see* the squares, dark and light alternately, and the pieces upon them more or less distinctly. In other cases it is rather more symbolic—*abstract*, as M. Binet terms it; the player sees only part of the board at a time, and represents in a vague way the pieces and their positions; but he works by means of the *potency* of the pieces,—an idea, rational rather than visual, of what each one can do from its given position. In addition to this there is in some cases a *verbal* memory; *i. e.*, an auditory memory of

the names of the squares and moves; a great many players repeat the moves in a low tone of voice, to re-enforce the visual memory-image.

A different phase of memory from those treated in this book, is taken up in a study of the visual memory of children; 225 school-children, from 9 to 13 years of age, being examined in groups of four. They were shown a line, and afterwards asked to reproduce it, or pick out one of similar length from a set. Three standard lengths, 16, 40, and 68 mm., were used. M. Binet does not mention the lapse of time between the original presentation and the choice or attempted reproduction (10 to 15 min. ?), nor the differences in length of the lines given to choose from in the second place. The absence of these data prevents us from comparing his results with those of a somewhat similar investigation recently conducted at Princeton, (using squares, however, instead of lines), which will be reported in a succeeding number of this REVIEW.

Among the laboratory studies are several on *audition colorée*. M. Binet and his colleagues examined a number of persons subject to this phenomenon, in order to determine the rapidity with which they associate the color with the word or vowel. So far as they go, the figures seem to show that this peculiar color association is not more rapid than other kinds of association; M. Binet shows by experiments on himself, that one who has not colored audition may *learn* a set of associations between vowels and colors so thoroughly, as to be able to call up the proper one in each case, and may do so even more rapidly than those who make the association naturally.

PRINCETON. H. C. WARREN.

RECOGNITION AND ASSOCIATION.

Assimilation and Association. (I and II). JAMES WARD. Mind, N. S., II, p. 347, July, 1893, and III, p. 509, October, 1894.

By assimilation, Dr. Ward means the recognition, or as he prefers to say, the cognition, involved in perception, that is, what Höffding names *unmittelbares Wiedererkennen*. His central position is the assertion that assimilation, in this sense, so far from being identical with association or explained by it, is really the necessary presupposition of association, which is obviously between *recognized* objects of consciousness. The most important part of the discussion is the vigorous criticism of the view that assimilation is really a case of 'association by contiguity' and that, accordingly, an object seems familiar through the presence of faint representations, *Nebenvorstel-*

lungen of events or objects, formerly connected with the familiar one. Dr. Ward's objections to this theory are based largely on physiological considerations. From Müller, Siemerling and others, he quotes cases of cerebral disturbance, in which "visual memory images are for the most part retained, so that old scenes can be recalled and familiar objects or persons accurately described, and yet the recognition of them is no longer possible, when such persons or objects are actually present." Dr. Ward insists, perhaps without sufficiently considering the possibility that apparent concrete visual memory-images may really be verbal images, that such cases would be impossible if sense-percept and sense-image were exactly the same. Hence he denies that "the seat of the ideas is the same as the seat of the sensations," though "of course," he adds; "it is not likely that there will be any wide separation . . . . both might even belong to the same convolution, though possibly to different layers of its cortex." Facts of brain change, especially the "evidence of the gradual maturing of the cerebral projection-system and of its priority to the association-tracts" are adduced to prove the physiological possibility of assimilation, when association is impossible. In his positive treatment of assimilation, Dr. Ward gives, as he says, 'no precise answer to the question,' but this is natural, for his tendency is to regard assimilation as an ultimate, and therefore unanalyzable, aspect of conscious activity. Assimilation is consciousness of familiarity, and the interesting parallel between cognitive familiarity and motor facility, or habit, suggests, at the outset, that the nature of familiarity "is to be formed rather in the subjective than in the objective constituents of consciousness." Quite in consonance with this premonitory remark, assimilation is, in the end, allied with attention of a 'spontaneous, selective and concentrated form,' and described as a form 'of subject activity and interest.'

The most curious feature of the essay, is Dr. Ward's persuasion that his theory is 'in the main' in agreement with Höffding's. In contending that such immediate recognition does exist, without necessarily involving contiguous association, both authors of course agree. But Höffding expressly makes the identification, justly repudiated by Ward, of assimilation with 'association by similarity;' and in the details of his theory, even in the later, modified form of it, he either, as Ward admits, "lays dangerous stress on the physiological effects of mere repetition," or offers a psycho-mechanical explanation differing inherently from Ward's emphatic insistence upon the preëminently subjective nature of assimilation.

MARY WHITON CALKINS.

LOGICAL.

History and Natural Science. W. WINDELBAND. Inaugural Address as Rector. Strassburg, May, 1894.

The current division of the experiential sciences into natural sciences and sciences of mind is unfortunate. Psychology does not fall exclusively in either class. For while as regards its object of study it is a 'science of mind,' its method is that of the natural sciences, viz., that of seeking *laws* of processes, whereas most of the 'Geisteswissenschaften' seek to set forth some *single* event or process in its historic relations. A better division from a purely methodological point of view would be into *natural* and *historical* sciences. The former seek universal laws, the latter seek particular historical facts. The goal of the former is the general apodictic judgment, of the latter the singular assertory proposition. The former may be called *nomothetic*, the latter *idiographic*, bearing in mind that the same material may often be considered from either point of view. Logic has always been under the influence of the nomothetic thought forms. It has centered its investigation about the universal judgment, and as regards the actual procedure of science to-day we have devoted far more attention to the theory of experimentation than to the parallel problems of historical methodology.

The results of these diverse methods are widely differing structures. History presents a world of living, concrete individuals; science leaves out all that is individual and constructs her mathematical formulations of the laws of motion, her world of atoms colorless and soundless. This at once suggests the question as to the respective values of the two results, especially as regards their influence on our views of the world and of life. Every object or fact has value in proportion as it contributes to a universal. The particular may fulfil this function, however, not only by being subsumed under a general concept but also by forming a significant constituent of a concrete whole. Man is preëminently interested in the individual and unique, especially in the sphere of personality. But on the other hand the idiographic sciences which portray this must employ general principles furnished by the nomothetic.

These two *moments* of human thought cannot be reduced to one. Causal explanation of historical events might seem to indicate a possibility of completely explaining the particular from universal laws, and so Leibnitz thought that ultimately all *vérités de fait* had their grounds in the *vérités éternelles*. But he could only postulate this for the divine thought, not carry it out for human intelligence. This

'irreducibility' may be illustrated by the fact that every event may be regarded as the conclusion of a syllogism requiring two premises,—one, the major, a natural law, the other the minor, an actual antecedent in time. Hence the individual can never be completely explained, *i. e.*, reduced to general laws. This is illustrated by the impossibility of a complete analysis of personality by general categories, where the irreducible element appears to us as the feeling of the causelessness of our own nature, *i. e.*, individual freedom.

J. H. TUFTS.

UNIVERSITY OF CHICAGO.

The Conception of Infinity. SHADWORTH H. HODGSON. Proceedings of the Aristotelian Society, Vol. II., No. 3, Part 1.

It is impossible to analyze and criticise satisfactorily in so brief a space as is contained in the limits of this review, Mr. Hodgson's interesting and closely reasoned paper on infinity. I can only give the outline of his argument and indicate what seem to me its strong and its weak points.

The term *infinity*, Mr. Hodgson maintains, signifies an attribute and not a substance. Since all attributes qualify the substance to which they belong, infinity must be a quality of the thing to which it belongs. But though infinity is thus a quality, it is, so to speak, a *quantitative* quality. In the wider sense of the word it is a quality but not in the narrower. All quantitative determinations of things are qualitative, in the broader sense of the term. There must, then, be something an essential quality of which is quantitative, making it capable of quantification.

The two senses in which the word quality may be used may be brought out in an illustration : we may distinguish in the experience we call a rose : (1) Certain specific feelings such as color, odor, etc. (2) Certain properties which are its quantitative elements, and (3) The fact that certain constituents of these two classes are combined in a particular way at a particular time—the 'existential' element.

The second class, or the quantitative constituents of the experience, are the source of our having the perception or the idea of quantity at all. There are two constituents of this class, time-duration and space-extension. Every content of consciousness must contain a constituent of class (1) and of class (2). The constituents of class (2) may be called the formal element of consciousness. This formal element alone is immediately and essentially capable of quantification, and is the source of all our knowledge of quantity.

Feeling is only quantifiable indirectly, in virtue of its occupying time, or both space and time.

Since, therefore, infinity is a quantitative quality, it is to time and space alone that it must in the last resort attach, if it exists at all ; and time and space are those entities or substances or things whose essential quality is to be quantifiable.

But looking at the panorama of our experience where we will, we find that divisions between feelings, or between feeling and absence of feeling, are always divisions which fall within time or space, never beyond them, *i. e.*, they are limits beyond which there are space and time again, whether this space or time beyond the limit is or is not occupied by a specific feeling or content. In other words space and time in their entirety are wholly limitless and inexhaustible.

Infinity is a fact of perception, observed alike in the minima and in the maxima of perception. Our perception of infinity must be carefully distinguished from our conception of infinity,—the latter being a single finite item in a whole hierarchy of similar conceptions. Our perception of the infinity of space and time is a fact of experience.

If what precedes be true, then we cannot but conceive the universe as infinitely extended. We perceive time and space to be infinite, and as the formal element of consciousness is inseparable from its material co-element ('quality' in the narrower sense), we must conceive existence as extending commensurably with time and space, beyond the boundaries of existence as positively known by us. Time and space, beyond the bounds of any content positively known to us, must be combined with some co-element or other, for it is only as a co-element that we know them.

The universe as positively known or knowable is a world of matter. The unseen world may be immaterial, and faith lays hold upon it, thus satisfying the demands of the moral nature, which positive knowledge leaves unsatisfied. This is the triumph of the Practical Reason, on the field upon which the Speculative has met with defeat.

Such is Mr. Hodgson's argument. I have given it only in abstract and I have eliminated certain portions, but I have not broken the chain of his reasonings. His analysis of consciousness into its formal and material elements is, I think, clear and masterly. His style is simple and without ornament,—a great virtue in a philosophical writer. His conclusion does not appear to me to be proved.

I shall not enter into a general discussion of the infinity and infinite divisibility of space and time, though I think his arguments to prove these are not above reproach, but I shall confine myself to pointing out that consistency would rob him of his world of faith, and shut him up to the world of matter, or such part of it as comes within experience.

He has said that the infinity of time and space are given in experience. Time and space, however, are, as he has also maintained, given in experience only as co-elements,—they are inseparable from the material elements of experience. It would surely seem to follow that if the infinity of time and space are perceptual facts, the infinity of the world in time and space must be a perceptual fact too. Can we have an experience consisting of a certain limited quantity of 'form' and 'matter' combined, and, extending infinitely beyond that, an experience which consists of but the one element, the formal? Are pure space and pure time without any material filling given in perception? If they can be, a world beyond the limits of the known world, a world of faith, cannot be assumed to fill the void, for the void can be conceived as unfilled,—no co-element is necessary. If, on the other hand, a co-element be really necessary, we cannot perceive time and space to be infinite, without at the same time perceiving that which fills them to be infinite, for they cannot be experienced alone.

Now Mr. Hodgson holds that the universe is not positively known to us as infinite. Its infinity is not a perceptual fact. Must we not thence conclude that the infinity of time and space, the formal element in experience, is also not positively known to us? Co-elements must be co-extensive, if they are really inseparable.

Mr. Hodgson must either hold that the formal element may be divorced from the material, in which case he cannot assume the existence of an unknown world to fill void time and space ; or that it can not, in which case he must admit that, if we positively know time and space to be infinite, we also positively know the existent world to be infinite. This makes an argument for its existence unnecessary, and rubs out the line between the worlds of knowledge and of faith.

I do not believe Mr. Hodgson can get his conclusion without blowing both hot and cold in his premises. He has certainly done so in this argument.

UNIVERSITY OF PENNSYLVANIA. G. S. F.

L'Abstraction et son rôle dons l'education intellectuelle. QUEYRAT. Paris, Alcan, 1894. Pp. 144.

This is a popular statement of classical ideas on abstraction, ideas to which the author has added, in order to rejuvenate them, many quotations borrowed from contemporary psychologists. The book merits neither praise nor blame; it may be useful to persons entirely unacquainted with psychology. It should serve to confirm others in the idea that those who make science are the ones who should take the trouble to popularize it. A. B.

SORBONNE, PARIS.

ANTHROPOLOGY.

Memoirs of the International Congress of Anthropology. Edited by C. STANILAND WAKE. Chicago, 1894. Pp. 375.

The prompt appearance of this volume (it was issued last spring) and the handsome style of its manufacture secure for it the first words of commendation, and they will certainly not be the last.

At the International Congress, of which it is the record, anthropology was interpreted in its widest sense as the science of man, in all directions of his development, physical and psychical. An effort was made to consider the history of the species as an aggregate, and to lay down the principles for its scientific analysis. The separate branches considered were physical anthropology, archæology, ethnology, folk-lore, religions and linguistics, and in the published volume two or more papers are given on each of these divisions.

While all of them are meritorious, a few deserve special mention on account of the extent of new observations, the results of which they present. In Dr. Franz Boas' article 'The Anthropology of the North American Indian,' and in that of Dr. Gerald M. West on 'The Anthropometry of American School Children,' we have condensed statements of the many thousand measurements undertaken by the Department of Ethnology of the Columbian Exposition. For the first time positive data of wide provenance are supplied in the two branches named.

Another paper replete with new facts is that on 'Primitive Scales and Rhythms,' by Prof. John C. Fillmore. It is a study of the musical powers of the American Indian by one deeply versed in the theory of the art.

Mr. Mercer gives an account of the discovery by himself of a flaked flint in ancient quaternary gravels in Spain; and Prof. O. T. Mason contributes an interesting study on aboriginal American

mechanics. Mrs. S. W. Stevenson illustrates a phase of primitive thought by tracing an ancient Egyptian rite; and Miss Alice C. Fletcher presents some Omaha love songs, showing that this sentiment is also one of those which are primitive. Two of the articles are in German, which is quite appropriate in an 'international' volume.

It is to be regetted that psychology proper did not find a place among the subjects discussed, or at least not among the papers printed. The science of anthropology is not a science when psychology is omitted, and the sooner its commanding position in the study of man is recognized the more rapid will be the progress of a sound knowledge of the species.

D. G. BRINTON.
UNIVERSITY OF PENNSYLVANIA.

EDUCATIONAL AND CHILD PSYCHOLOGY.

The Proceedings of the International Congress of Education of the World's Columbian Exhibition. Held in Chicago, July 25–28, 1893. New York, National Educational Association, 1894. Pp. XVIII.+1005.

The extended series of volumes representing the proceedings of the National Educational Association since its formation, in 1857, is certainly one of the dreariest collections of pedagogical words and phrases to be found in our language. The volumes will in no way stand comparison with the proceedings of the American Institute of Instruction. The present thick volume of more than a thousand pages goes far, however, toward redeeming the whole series. It gives an encyclopedic view of the educational theories of to-day as presented by some thousands of representative men and women from all over the world in a three days' educational congress. It is not generally understood that there were in Chicago during the summer of 1893 two educational congresses meeting in the same rooms on successive weeks, and each divided into a score of sections covering essentially the same ground. The first congress, July 17th to 25th, was under the direction of the Woman's Branch of the World's Congress Auxiliary, and its proceedings have never been published. The second congress was under the direction of the National Educational Association, July 25th to 28th, and the present volume gives most of the papers presented, with brief notes on the discussions.

One is struck in reading this volume, as in reading all the teachers' literature of the day, with the fact that hardly any of the papers deal with what has been, or with what is; they all struggle with the

question: What ought to be? The exceptions are to be found in the papers of some of the foreign delegates who describe work actually being done in their own countries, and in the work of the psychological sections. It would seem that on a great historical occasion like that which drew the congresses together it might have been expected that some effort would be made to gather up the results of our past pedagogic experience, and then from the historical point of view forecast the probable future. There are very few papers written from this point of view, but most of the discussions are purely theoretical.

The work of the section in Rational Psychology fills some thirty pages, and includes studies by such representative men as Dr. McCosh, Prof. Royce and President Schurman. Cynics who charge us pedagogues with being men of narrow intellectual interests will find sufficient breadth of view in this section to reverse the charge.

Of most interest to readers of this review will be the seventy pages devoted to the work of the Congress of Experimental Psychology in Education. Two years before, at its meeting in Toronto, in 1891, the N. E. A. had given some attention to this line of study through two round-table conferences, presided over by G. Stanley Hall, though they were not recognized as a part of the regular association work. Again, in 1892, at Saratoga, two round-table meetings dealt with experimental psychology, but the meeting in Chicago was really the first great educational meeting in America where any considerable time was set aside for the direct study of the original stuff with which all educational theory deals. The papers at Chicago present special studies on physical development, stuttering, imagination in childhood, children's language, children's theology, eye and ear-mindedness, the psychology of reading and spelling, reports on work being done in different parts of the world, with several more general studies showing the relation of this sort of work to educational theory and practice. Such well-known men are represented as G. Stanley Hall, Wm. Burnham, James Sully, E. M. Hartwell, Francis Warner, and Wm. Bryan. There is no other single place where so much material has been brought together bearing on the study of children as in this volume, with the exception of the files of the Pedagogical Seminary.

The volume must prove to be a valuable and permanent reference book for students interested in educational theory and the beginnings of educational psychology.

EARL BARNES.
STANFORD UNIVERSITY.

NEW BOOKS.

Lehrbuch der Psychologie. W. VOLKMANN. Vierte Auf., Bd. I. Cöthen, Schulze, 1894. Pp. vii + 511.

Peregrinazioni psicologiche. TR. VIGNOLI. Milan, Hoepli, 1895. Pp. 404.

Filosofia morale. L. FRISO. Milan, Hoepli, 1893. Pp. xii + 335.

From the Greeks to Darwin. H. F. OSBORN. New York, Macmillan & Co., and London, 1894. Pp. x + 259.

Lectures on Human and Animal Psychology. W. WUNDT. Translated from the second German edition by E. Creighton and E. B. Titchener. London, Swan, Sonnenschein & Co.; New York, Macmillan & Co., 1894. Pp. x + 454.

Saggio di una scala normale del pensiero astratto. S. DE CRESCENTO. Naples, d'Auria, 1893. Pp. 27.

Principii di logica reale. N. R. D'ALFONSO. Rome, Paravia, 1894. Pp. 71.

Genie und Entartung: eine psychologische Studie. W. HIRSCH. Berlin u. Leipzig, Coblenz, 1894.

Zur Analyse des Apperceptionsbegriffs. J. KODIS. Berlin, Calvary, 1893. Pp. 202.

Introduction to Comparative Psychology. C. LLOYD MORGAN. Contemporary Science Series. London, Walter Scott; New York, imported by Charles Scribner's Sons, 1894. Pp. xiv, 382.

Entwurf zu einer physiologischen Erklarung der psychischen Erscheinungen. I. Thl. SIGM. EXNER. Vienna, Deuticke, 1894. Pp. viii + 380.

Studies from the Yale Psychological Laboratory. II. Edited by EDWARD W. SCRIPTURE. New Haven, Conn., Yale University, 1894. Pp. 124. $1.

La logique sociale. G. TARDE. Paris, Alcan, 1894. 7 fr. 50.

Mémoire et imagination. LUCIEN ARRÉAT. Paris, Alcan, 1894. 2 fr. 50.

Les états intellectuels dans la mélancolie. G. DUMAS. Paris, Alcan, 1895. Pp. 144. 2 fr. 50.

Les lois psychologiques de l'évolution des peuples. G. LE BON. Paris, Alcan, 1894. 2 fr. 50.

NOTES.

Ex-President James McCosh of Princeton College died in Princeton on Nov. 16.

Professor O. Külpe has been called from Leipzig to the University of Wurzburg.

Professor Sorley has been called to the University of Aberdeen.

The publication is announced of Prof. Ribot's lectures at the *College de France*. The volumes, of which there will probably be three, are now in press for early issue.

Mr. A. H. Lloyd has been made Acting Assistant Professor of Philosophy, and given charge of the department for the year, in the University of Michigan. Mr. J. Bigham, Ph.D. (Harvard), and Mr. Geo. Rebec, Ph.B. (Michigan), have been appointed instructors in the same institution, the former to direct the work of the psychological laboratory.

Dr. W. R. Newbold has been appointed Assistant Professor of Philosophy in the University of Pennsylvania.

Dr. S. Mezes has been appointed Professor of Philosophy in the University of Texas.

Dr. Margaret Washburn has been appointed Professor of Philosophy and Psychology in Wells College.

Dr. W. B. Elkin has been appointed Professor of Philosophy in Colgate University.